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# INDUSTRIAL RESEARCH

*by*

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# FOREWORD

THE author, by his own confession, states that he is a perfect example of a pure scientist gone wrong. Starting a long time ago, as a mathematician and logician, he has progressed through physics to chemistry and the teaching of thermodynamics. Then, he organized, for the U. S. Navy, the Divisions of Chemistry and of Thermodynamics at the Naval Research Laboratory. Here he devoted himself to the invention of many useful devices. Later, he went into consulting practice, mostly in the fields of air conditioning and refrigeration. He always took great pains to go with companies where the management knew less about chemistry than he did and so was moderately successful in inventing in part the Faraday system of refrigeration, the Kathabar (Lithium Chloride) system of air drying, the B and R system of air conditioning and many gadgets of lesser importance. He was also connected with the early work on Freon. Next, he was sales manager, chief engineer and pretty nearly everything else in a small air conditioning business founded on his own inventions. He has organized and been in charge of three research laboratories and is acquainted with more others than he likes to admit. His hobby, I hope you have guessed, is writing. Two published books: One with Dr. Rossini on Thermochemistry, and one entitled "Is the Navy Ready?"

He was also one of the editors of the International Critical Tables. He has a couple of unfinished novels for which some day he hopes to find a publisher. A rolling stone gathers no moss, but it sure gets experience.

H. Bennett

# PREFACE

THERE are no new lands for America to exploit. It is probable that there are but few new mines and oil deposits yet to discover. There is left but one possible source of new wealth to replace the wealth lost in war. This source of wealth is the discovery of new products to manufacture and sell. It is on this source of wealth that we must now and in the future depend for the means to maintain and raise our standard of living.

It is from the research laboratory that this new wealth must arise, as has originated in the past, the new wealth represented by the automobile industry, the radio industry and the industries founded on the movies, the electric lamp, the aeroplane, plastics, rayon, Nylon, Kodak and hundreds of other new products. This wealth has never been fairly estimated but it probably represents well over two hundred billion dollars.

Industrial research is thus a matter of extreme social and national importance. Even the most hard headed business and banking executives are beginning to see this.

Beginning less than fifty years ago with nothing, there are now over two thousand industrial laboratories spending over a quarter billion dollars a year. Research has become a sizable industry all by itself.

Most of these laboratories have developed within the organization by a sort of internal concretion like pearls,—or

boils,—caused by some accidental irritant. Growing as they have, without reference to the experience of others, some of the laboratories have proven to be pearls of great price indeed, some just a "pain in the neck." This is inevitable, in the first stages of development of a new business tool. Mass production first went through this phase, as many of us remember.

But now a stage has begun in which it is being understood that research sometimes pays and sometimes doesn't, and that there are more or less general rules of management and organization which make the difference between a pearl and an irritant.

The purpose of this book is to display the social importance of research and to outline those general principles of management and organization which have proven successful in the laboratory.

This book, therefore, may be considered as a manual for the research director and for the business executive, real or potential. But it is more than that, as it presents in a concrete form what may be thought of as a philosophy or theory of research in its social aspects.

It is based on considerable personal experience in laboratory management, but chiefly on the accumulated experience of laboratory executives all over the country who must be nameless, as I have wanted the freedom to say things about their laboratories for which they should not be held responsible. To these men I give my thanks.

F. R. BICHOWSKY

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## Chapter I

# RESEARCH THE DESTROYER

The real importance of industrial research in national life is constructive, not destructive. It is a mine from which new sources of national wealth are constantly being uncovered. It is on these new sources of wealth that the economic future of democracy must rest, not on conquest or enslavement.

As such, industrial research is worthy of serious study by all economists, and for that matter by all who are interested in the future of the state. More and more, in the future, we must depend on new sources of wealth discovered by research to supply the means of maintaining and increasing our standard of living—indeed our entire social life.

But, to understand in any detailed sense what industrial research is, it is convenient to begin by studying the destructive power of research, just as it is prudent to understand the destructive power of dynamite before attempting to use it for constructive purposes.

The destructive power of research is very real. There is no menace which can be as deadly to an established business as industrial research. No war, no panic, no bank failure, no strike or fire can so completely and irrevocably destroy a business as a new and better product in the hands of a competitor

The history of modern business is packed with instances where a well-established, well-managed conservative business

has gone into bankruptcy or disastrous reorganization because a competitor has entered the market with a new product.

In 1850 there were over twenty-five hundred miles of canals completed or under construction in the United States. Nearly a thousand additional miles were projected. Four different canal systems were built or building to cross the Alleghenies. These were the James River system, the Chesapeake and Ohio system by way of the Potomac, the Pennsylvania system by way of the Juniata, and the Erie canal.

Four different systems of canals were to connect the Great Lakes with the Ohio and Mississippi. Canals were to connect Boston and Albany, Pittsburgh and Rochester, Syracuse and Harrisburg, the Hudson River and the Delaware, the Delaware and the Susquehanna, New York and Charleston, S. C.

Over a hundred and fifty million dollars had already been invested in this business. In terms of national wealth, this represented a proportionate investment equal to an expenditure of seven billion dollars at the present time. Most of this money was put up by the states,—their first essay in business (unfortunately not their last), but some was invested by private corporations. State and private canal bonds were eagerly bought by the banks and by the investing public. Why not? The canal was the cheapest and quickest mode of transportation of heavy goods. Nature had protected the investment as there were only a few possible routes and competition would therefore be almost impossible.

The advantages of canal transportation were great. A farmer could cut trees from his wood lot, build a crude flat-bottomed barge, load on his household goods, attach a team of horses and move himself, family, household goods, stock, food and fodder to any point in the great midwest at a cost which was fantastically low.

Even though now nearly forgotten, these canals had a great social significance. Tens of thousands of people travelled to the west this way. It was by this mode of transportation that Indiana and Illinois were mainly settled. Towns, cities and factories sprang up along the canals to take care of this travel. Rochester, Syracuse, Akron, Dayton, and Ft. Wayne owe their original growth to the canal.

Of course there was being tried out another newfangled, dangerous, expensive, uncomfortable, noisy, smelly, dirty method of transportation by some fools down Baltimore way, but any one (in 1850) could see that the steam railroad was just a fad and never could compete with the canal.

Under the glass top of the desk of every executive and chief engineer should be a copy of the carefully documented arguments by which the State Engineer of New York conclusively proved that the railroad could never compete with the canal. The following passage from McMaster's "History of the People of the United States" gives the same argument in more popular language:

> "Canals . . . are facts; railroads are theories, and are opposed to the habits and feelings of our people, for they create monopolies in transportation. A farmer cannot own railroad wagons. But for a few hundred dollars he can buy a boat, or with the help of his hands can build one to carry twenty-five tons. To move such a load by railroad would require eight carriages and a locomotive costing $4,000. Into his boat the farmer can put an assorted cargo of flour, bacon, hemp, plank, lumber and vegetables, draw it to market with his own horses, sell it at any village on the way and bring it back loaded with what he pleases. Does anybody suppose railroads will take on loads offered anywhere along the line? No, indeed! The farmer must haul them to the stopping

places. Canals will carry livestock, hay, firewood, large timbers for ships, building boards and planks. Railroads cannot do this. What would be thought of a load of hay coming along a railroad? The sparks from the locomotive would set it on fire before the journey began. Canals are adapted for military purposes; railroads are not. Imagine a regiment of troops with baggage, provisions, ammunition and camp equipage transported by railroads! By canal this can be done and the soldiers live and cook comfortably on the way. The boats will carry tents, foods, baggage and ammunition and may be drawn by the horses or by the men as they walk along the tow-paths. Canal boats will carry artillery which cannot be transported by rails unless the guns are dismounted and the caissons taken apart. Snow will make a three hundred mile railroad impassable for weeks. Rain will wash earth over the rails in quantities which in deep cuts will take weeks to remove. Railroads for long distances are wholly untried in any country and for short distances are yet in the experimental stage. The longest in existence, the Manchester and Liverpool, is but forty miles in length and passes through the heart of a populous country and may anywhere get aid to repair cars, wagons and engines. But such a railroad as the Baltimore and Ohio ten times as long running through a rough, wild and sparsely inhabited country with great difficulties of construction to overcome should ever compete with a canal of the same length as the Chesapeake and Ohio surpasses probability.

If steam locomotives were used it would be necessary to have water-boiling stations every six or seven miles to furnish the engines with tanks of boiling water for a supply of cold water would stop the generation of steam and stop the train.

> Rails would be broken by passing teamsters wantonly as they did milestones and coping of bridges, or from spite towards a means of transportation likely to injure their business. In the mountains the cold of the winter is often so severe that an axe will break when struck against a tree. Would not rails snap under these conditions as a train passed over them?"

How often has every promoter of a new device heard arguments of the same family!

Yet in spite of these arguments in 1860 the canal was dying and by 1870 it was most certainly dead. The total loss to the states by their first adventure into business has never been fully calculated, but it was over a quarter of a billion dollars, and in 1860–1870 that was a huge amount. The indirect loss was surely much greater as the default of the canal bonds was one of the causes of the panic of 1856.

So in 1910 the railroad executives who never learn anything sat back and said, "Now *we* have a perfectly safe business. No outside competition can touch us; all we have to do is to sell more bonds, build more railroads, collect more profits. There can be no competition to a railroad except another railroad; the experience of the canals proved that, and if the B. and O. tries to get some of our business away from us and puts on another train to New York at 5:05 P. M., why, all we have to do is to put on a competing train to New York at 5:05 P. M."

Our fictitious railroad executive in 1910 had every reason to feel content with himself. The growth of the American railroads had been phenomenal and yet extremely regular In 1860 there were 30,000 miles of track, in 1880 there were 95,000; in 1900 there were 193,000; in 1910 there were 240,000. His statistician had calculated that by 1940 there would be 292,000 miles of railroads.

In 1880 the railroads employed about 500,000 persons; in 1900 about 1,000,000; in 1910, about 1,700,000. You could easily calculate that in 1940 there would be 2,300,000 employees.

In 1910 the market value of the stock of all American railroads was 8.3 billions and the value of the bonds about 12.0 billions. His statistician could calculate that by 1940 the bonded indebtedness would be about 14.7 billion dollars and the value of the stock would be about 10.4 billions; quite a neat profit, not to mention about 12.0 billion dollars in dividends.

Now, of course, in 1910 our railroad executive was aware that some "crackpot" mechanics, out in the middle west, had invented a gadget called the automobile. Being a sporting fellow, and interested in all things mechanical, our railroad executive owned one himself. Occasionally he even attempted to drive down town with it on a Sunday afternoon when he had lots of time, that is, if the weather was favorable. But to imagine that these monstrosities would ever compete with the Pullman car was as absurd as that fool report that two bicycle repair men out in Dayton had learned how to fly.

Yet in 1940 the actual railroad mileage was 238,000 a loss of 15,000 miles from the peak and a loss of 54,000 from estimate. Employment had dropped to 1,000,000,—less than half of the employment at the peak. The market value of the railroad bonds was less than 8 billions, and our railroad executive, if he had saved his pennies (and lost his sanity), could have bought on the open market the complete stock ownership of the American railroads for 2.2 billions of dollars.

This gigantic loss was due to a variety of causes, but primarily it was due to Mr. Selden, Mr. Ford, Mr. Haynes and a host of other inventors who contributed, one by one, the inventions that make up the modern automobile. There

must be charged against the tool-shed and the laboratory, in which the automobile was born, an economic loss of at least 10 billions of dollars to the investor in railroad securities, a loss of employment of a million persons, and a loss in income of at least 30 billions.

This is an example of real technological unemployment and real technological capital loss. Most technological losses are fictitious as improvements of efficiency of manufacturing by use of "labor saving" devices increases employment. But this is real, for the loss of capital by the obsolescence of a business is real loss of capital of which nothing can be salvaged. The loss of employment is not quite so absolute, as in the case of the railroads. The men thrown out of employment have had no training except in that particular kind of business the readjustment is always difficult and in some cases impossible.

This technological loss is directly chargeable to the research laboratory, or, more fairly, to the management which failed to insure against this loss.

The railroad and canal industries are not isolated cases. There is hardly an industry which does not afford illustrations of the destructive power of research. The cotton industry, for example, where synthetic fibres (Rayon, Nylon, etc.) are to a large extent chargeable with the economic loss represented by falling cotton prices. One can go on and on, but to do so would be to emphasize unduly the red side of the ledger of research.

To understand the black side of the account consider the illumination industry. In 1750 the perfect illuminant had been found. This was the blubber and head oil of the spemaceti whale. From this was made the whale oil used in lamps and the spermaceti used in candles. Under the impetus of this market, whaling became one of the great industries. In the United States cities like New Bedford, Edgartown, Nan-

tucket, Salem and Gloucester grew in wealth and power. Moby Dick is an adventure story; it is also the story of an industry now almost forgotten in America.

The reason why this industry has been forgotten is partly because the unrestricted whaling greatly reduced the supply of whales, but far more because in 1815 a Frenchman by the name of Chevreul happened to boil soap fat with lime rather than with lye and thus produced a mixture (useless for soap) of glycerine and an oily layer which later hardened. He called this oily layer, which we now know was a mixture of fatty acids, "Stearine". Gradually stearine candles which do not soften in hot weather replaced spermaceti candles, as the latter had replaced the wax and tallow candles of an earlier generation.

Then once more a new business cycle began. In 1859 Drake brought in the first oil well. The thick, heavy, smelly oil thus produced had at first little use and was sold under the name of rock oil or petroleum to rub on bruises. For a while Drake wondered where he could sell the thousands of barrels of oil that his well produced. Then, driven, as is so often the case, by the necessity of increasing the market of a product already under production, he sent some oil to the S. M. Kier Refinery of New Jersey, which manufactured shale oil, with a request to distil some of the oil. Drake probably had hopes that he would sweeten the odor of the oil, remove some impurities, and possibly improve its quality as a grease.

On distilling the oil in the crude pot stills that remained in the industry for fifty years, it was found that the oil did produce a good lubricant. There were also large quantities of two lighter fractions; one was so volatile that it could not be safely shipped. The only thing they could think of to do with it was to burn it as a substitute for gas under the still. For this reason they called it gasolene, hoping indeed that it might have some use elsewhere as a substitute for gas.

There was also a light non-volatile fraction which smelled badly but which would burn in the wax oil lamps of that day. They called it "kerosene" meaning "wax-like", hoping that the name would fool the customer, who was accustomed to think of wax oil as the ideal illuminant.

With the discovery of kerosene, the wax oil industry was destroyed, and on its ruins was erected the Standard Oil Company, the prototype of the petroleum industry of the present day.

Memories are so short that people are inclined to think of the oil industry as connected with the automobile in some way, but actually it was founded to produce "oil for the lamps of China". As far as the oil industry was concerned, the discovery of the automobile was simply a fortunate accident which saved the life of the industry at a time when it too was threatened with extinction.

In 1875, just when things were doing best in the petroleum industry, a hare-brained inventor with no business sense at all was blundering his way along with the purpose of making an arc light, small and simple enough so that it could be used in the house. He was never one to profit by the experience of others, so that he repeated, one after the other, almost all of the errors which other inventors had made before him. Finally, having tried everything in vain, he convinced himself, the hard way, that the only way was to make a filament fine enough so that it would glow when a current passed through. But even in a vacuum all the filaments that he tried, burned out in a few minutes. Others had gotten this far, but at this point they had stopped—discouraged. He plodded on, trying everything he could think of, no matter how foolish. This is a method of discovery which modern scientific investigators look down upon and are prone to call, in derision, "the Edisonian method", but sometimes it works.

After thousands of experiments he happened to use, in his

vacuum bulb, a fiber of carbonized bamboo. Thus was born the incandescent electric lamp, and thus was destroyed the kerosene industry of the United States. Indeed it would have been hard to keep the whole petroleum industry alive except that with the automobile was found a new use for the despised waste product gasolene.

As has been said, Edison was no business man, and so in the course of time he lost control of the production of lamps and of the machinery to light them; and a group of Lynn shoemen, seeing a speculative opportunity, picked up the fragments of the industrial group which Edison had founded, combined them with a small manufacturing company of their own, and thus created the General Electric Company.

Once more the cycle turned. This time something new happened. By 1899 Edison's original patents had expired, and Dr. E. W. Rice, the chief engineer of the company, urged by the patent attorney Albert G. Davis and abetted by that emigrant hunchback Charles Proteus Steinmetz, took the unprecedented step of hiring half-time a young University professor, Willis R. Whitney, to head an experimental laboratory. Presumably the idea was to do something to retain the lamp business, which was threatened by competition now that the Edison patents were expiring.

There had always been certain difficulties with the carbon filament lamp. After a time it blackened and burned out, due to the distillation of carbon from the hot filament. Then, too, the carbon lamp was a hog for current. Only about three tenths of a percent of the current paid for was converted into light. There were two things which obviously might be tried: one was to treat the filament so that it would be more efficient; the other was to find some substance better than carbon from which to make the filament. Whitney thought that it would be more practical to treat the filament, so he tackled that first, and in due time discovered a method

of treatment of carbon filaments which greatly prolonged their life, and increased their efficiency. The lamp thus produced was known as the Gem lamp.

The second task was more of a gamble, so it was put off for a time. The substance from which the proposed filament must be made had to be a metal to conduct the current, and had to have a high melting point to stand the high temperatures of the lamp.

A moment's reference to a textbook would show that there were only three substances that could possibly be used: osmium, tantalum and tungsten, and of these osmium and tantalum had already been patented by a competing interest. Besides, tungsten would be better than tantalum, if only it could be made in the form of a wire.

Dr. Whitney has said in a public address that he is just a plodder, not in the least a genius, and, in a sense, that is true. It is certain that any one of a thousand American chemists placed in Dr. Whitney's position would have hit on the use of tungsten. In fact others did, but it took plodding and something else to keep at it when it was found, after hundreds of trials, that tungsten was too brittle to use and could not be drawn into a wire.

The man who actually discovered the steps of making tungsten ductile was Whitney's assistant, Dr. William D. Coolidge, another college professor with no practical background.

With the discovery of the tungsten lamp another cycle of progress was completed. The carbon lamp gradually passed out of production and the tungsten lamp took its place, as whale oil had taken the place of tallow, as stearine had taken the place of whale oil, as kerosene had taken the place of stearine, and as the carbon lamp had taken the place of the kerosene lamp. To you this meant that your light bill was less than a tenth of what it was before. But note the vital

difference: no men were put out of work, no factories were closed down; there was no loss due to the failure to meet bond payments, and there was no loss due to decrease in value of stock. Instead there was a great speculative increase in the value of the stock of the General Electric Company representing wealth gains of about half a billion dollars, not counting the very considerable absorption of capital in plant enlargment.

Note one thing further: during this early transfer period, while the carbon lamp was decreasing in sale and the tungsten lamp was beginning to be sold in large quantities, there was an actual loss to the company in the lamp department. This is apt to be the case with every new product during the period of development. Because with any new product costs are high at first, overheads are disproportionate, sales are low, sales costs and promotional expenses are high, and losses due to rapid changes in process, apparatus and specifications are heavy.

This period of adjustment is long since over, and the manufacture of tungsten lamps has become a large and very profitable industry, much bigger than the carbon lamp business ever was, though there are signs in the offing that it too may be nearing the end of its cycle.

I don't know to what account was charged the $2400 which was Dr. Whitney's first year's salary, but I am willing to bet that it was not charged to the correct account, that of insurance. For, in the first instance, that is what research is to an industry: insurance against capital loss, which in this case might well have amounted to a quarter of a billion dollars. Indeed, it is a loss which might have led to bankruptcy, for my understanding is that it was the lamp business which, more than any other, kept the company alive during the "depression" period from 1930 to 1933.

Of course this was not the only value of that first tiny

filament of tungsten which Dr. Coolidge held in his hand in the fall of 1908. The tungsten filament did not merely protect the company's investment in facilities for manufacturing carbon lamps. This little piece of fine wire was worth far more than all the carbon lamps which it displaced, for just as the railroad was a bigger business than the canal, and the automobile is becoming a greater business than the railroad, so the business created by the new and better lamp was much greater than that which it superseded. This is a law of research: every new product produces wealth greater than it destroys, for the human need or desire which made it possible for the new to displace the old gives to the new a greater market than the old ever enjoyed.

## CHAPTER II

# RESEARCH THE CREATOR OF WEALTH

THERE are some who paint a gloomy picture of the economic future of this country. They point out that the enormous growth in wealth and prosperity of the country between 1800 and 1900 was largely due to the discovery and utilization of new natural resources. The gradual increase in the acreage of farm and forest land due to the discovery of huge deposits of coal, iron, oil and gold have accounted for an increase in national wealth more than enough to take care of the losses in natural wealth represented by unproductive enterprises, wars and taxes. So great has been this increase in natural wealth that there has been sufficient surplus to allow the accumulation of excess capital and yet leave enough for important increases in the standard of living.

These economic pessimists point out that, in the future there will be, on this continent, no new lands to exploit, and that it is extremely unlikely that there are any very great mineral deposits yet to be found within the United States. Hence, they argue, the United States must look forward to a period of stable economics to be followed by a long period of decline.

The picture they paint is indeed a gloomy one, and one unfortunately all too true. There is but one ray of light to brighten it. They are right up to a point. In order to main-

tain civilization, in America, at its present high economic level, as well as to allow that level to rise, it is necessary that new frontiers be passed and new lands opened. What they forget is that there are frontiers to the mind and that there are vast unexplored fields of science awaiting discovery and exploitation. There is wealth to be found in these fields far greater than has been found in the prairies of Nebraska or the gold mines of California.

This is not merely a fantasy or wishful thinking of a specialist in research. The total value of the gold produced in California was about one billion dollars, while the increased value of the stocks and bonds of the automobile companies far exceeds that sum. Nebraska has added to the net available capital of the United States about another billion dollars, according to rough estimates,—only a fraction of the amount of new capital created by Edison's discoveries in the field of electricity alone.

Look at it in another way. If we exclude land, which is not really capital at all, it has been estimated that at least half of the entire capital of the United States is invested in businesses founded on inventions made in the last century and a half. To justify this seemingly extreme statement, look over any list of business concerns, say those listed on the stock exchange,—for example, American Can, American Radiator, American Sugar, American Telephone and Telegraph, Baldwin Locomotive, Bethlehem Steel, Borden, Chrysler, DuPont, Eastman Kodak, General Electric, Gillette, International Nickel, Lehigh Cement, National Cash Register, Paramount, Radio Corporation, Wesson Oil, Westinghouse, Pennsylvania Railroad, and Pacific Gas and Electric, to name but one representative from each group.

It is really hard to find a modern manufacturing business whose product, or at least whose manufacturing or operating method, is not the result of invention. Research therefore,

in some sense, produces wealth and is of value both in a financial and in a social sense. However, it requires a little closer consideration to translate these general facts regarding the economic and social value of industrial research into terms which an accountant or business executive can understand, and it is important to do this, as it is precisely in this detailed treatment of the financial value of research that one discovers some of its most significant social characteristics.

Probably, at some time or another, nearly every research executive has been asked to prepare a statement showing how much the research laboratory has contributed to the profit of the company. Most of them have attempted to comply with this request by preparing a statement showing the total or annual profits accruing from the sale of products invented in the research laboratory. But any good accountant could tell them that there is no necessary connection between the value of the inventions and the profits produced by their sale.

Increases in wealth always mean an opportunity for exploitation. Business, like real estate, has its increments. The most important is the increment which comes from the rise in value of the stock over and above the value of the physical assets of the company. What research does for a company is, in the last analysis, to contribute to this rise in value of the stock of the company. In the strict sense this increment to the value of the stock of the company, like the increment to the value of real estate, is unearned; that is to say, the value of the inventions of a successful research laboratory are disproportionate to the money and labor expended on the invention. These increases in value always give to the man or company furnishing the capital a chance to make a great profit, normally by the rise in the price of the stock, but often by means not so innocuous.

But the fact that money can always be made by the ex-

ploitation of a successful invention, does not mean that the business founded on such inventions will be able to operate at a profit. Thus radio and aviation stock, whose value has increased so enormously, has up until recently not been able to pay dividends.

As in other forms of business, there is no necessary connection between the creation of excess wealth by research and the earning of profits. A company like the Haynes Automobile Company, the DeForest Company, or the Isco Company, may by its research create an industry and yet go bankrupt or fail to earn a profit for its original owners.

The rather common failure of research to make a profit for the inventor and early promoters is not due entirely to the notorious (and exaggerated) lack of business ability of inventors. It is more often due to two facts which are commonly overlooked by inventors and their backers.

The first of these facts is that the creation of wealth out of these potential values requires capital investment larger than the wealth created. This is because the promotion of an invention requires a capital investment for business and equipment sufficient to supply the demand which the invention creates. If the invention is wanted by more customers than the inventing corporation can supply with its present plant and capitalization, competing concerns will spring up to supply that demand. This will happen regardless of artificial aids to monopoly such as patent protection, trade secrets, and the manufacturing advantages which accrue to the original manufacturer.

In the case of major inventions, this law of the obligation of business to care for the customer places such a large demand on capital that only in the case of the Bell Telephone interests has a private inventor and his associates been able. at the same time to finance the company, to supply the demand, and to maintain monopoly without loss of control.

The case of the American Telephone and Telegraph Company is exceptional because the value of the service to the customer is so largely dependent on the completeness of the monopoly.

The second handicap to profits is the fact that the original invention, if successful, is invariably a stimulant and a challenge to thousands of research workers elsewhere. Every second rate inventor in the country will proceed to work, in particular in the research department of those corporations whose business will be affected by the new invention. As a result of all this one of two things will happen: either an entirely new way of accomplishing the result of the first inventor will be found which is so much better than the original way that it will supersede the new invention entirely, or, still keeping the fundamental process of the invention, improvements of construction, performance or cost will be made which will make the invention in its original form unmarketable.

According to the patent law, these improvements cannot be used without license from the original inventor, who, it is supposed, has obtained a basic patent on his idea. But likewise the original inventor cannot use these improvements without a license from the men who made them.

As a result of these improvements within a short time, usually less than five years, the patent protection of the original inventor becomes useless since the public will demand the improved product. The original inventor cannot make this without the permission of the secondary inventor.

To see how these principles work against the small inventor take an imaginary case (to avoid hurting feelings). Consider yourself the inventor of a new television set which you can sell at a profit for $50.00. If you are a most unusual inventor you may be able to beg, borrow or steal a million dollars. Suppose with this capital you start manufacturing.

With this small capital you would have no means left for national advertising and no capital to build up a national selling organization. Your business would at first, at least, be small, and your distribution local. If your business is soundly managed, if you have luck, you might make ten to fifteen per cent on your investment. However, your sales would not greatly increase from year to year, because you cannot advertise and would have little free publicity. If your product is a good one, or even if it isn't, the local agent for the largest radio company would hear of it, and as every sale you made would interfere with his business, and, worse, with his sales morale, he would howl long and loud to his parent organization. Then the ponderous machinery of sales, sales engineering, and service would demand, "Why haven't we such an apparatus?" and the order would come down, from president to chief engineer to television division, "Produce a better and cheaper one", and in a surprisingly short time the television division would do just that, because almost any expert can improve on a product which already exists. The patent department would by this time have looked up your patents to see if they are perfectly valid. Probably they would not be, but suppose they are. That wouldn't deter them. They would go right to work and pretty soon they would have a series of patents covering all the improvements on your product of which their much larger research department could think. Now if they were an unusually considerate concern they might offer to buy your business, say for $750,000. If you were wise you would accept. If not, you would indignantly turn the offer down. But that wouldn't make any difference; they would go right on manufacturing, and would start selling their new television set at $49.50. This wouldn't hurt your business any. In all probability their free advertising would at first help your business. Nevertheless it would make you angry and you would sue them, where-

upon, if you really have a valid case, they would offer to form a patent-holding company to which you would give your patents and to which they would give theirs. At least they would have done this prior to the recent court decisions on patent pools. If you were wise you would accept; if not, you would go on with the suit. In due time, say four or five years, you would, if lucky, get judgment against them and you would be awarded, in back royalties, about enough to pay your lawyer. This would be gratifying, because you would figure the future income they would have to give you. But, unfortunately, by this time you would find your original product entirely out of date. Your customers would be used to the better product your rival was selling, and would demand that you too make use of the latest improvements which he has contributed to the art. They would want his set because it produced pictures in color, because it gave a clearer picture, and got greater distance, improvements which, with his much larger organization, he has been able to make. To meet this demand you would have to make an agreement to use his patents or go out of business. In any event you would lose your monopoly, because he would require the right to use your patents as a condition for granting you his patent rights.

Invention, then, even though it may make wealth for someone else, does not necessarily make a profit for the inventor. Profits in invention, as elsewhere, depend on management. There is no short cut for a sound business policy, and though sound business must include research and credit it with the increases of wealth it creates, it is a mistake of accounting to credit to research the indirect profits which may be earned on the wealth so produced. Likewise it is equally improper to consider a research department of no value to an organization because its products do not at first sell at a profit. The true measure of the worth of a research laboratory to a com-

pany is the increase in wholly owned wealth (increase in value of stocks less increase in bonded expenditures). It is extremely difficult to figure this in any given case. This increase in wealth is often huge even in cases where no dividends are being paid at all. Witness the increase in stock value of airplane and radio companies.

But any accountant will tell you that this is a speculative increase, and so it is. From the strictly business point of view the value of research is the increase in speculative value of the company. It is the potential unearned increment of wealth, called speculative value of a corporate holding, which in manufacturing companies is the true measure of the value of research. No one in his right senses would buy stocks yielding 2% when the rate on industrial bonds is 4½%, unless he believed that the future increase in value of the stock would more than recoup him for his loss in interest.

From another point of view, research may be considered as a mortgaged security. This is literally so, for if an invention is to produce income, capital is required to purchase the new machinery, goods and buildings. This capital must either be borrowed from funds which would otherwise go into dividends, or must be borrowed from banks, or must be borrowed by new issues of stocks and bonds. Banks, if they were really half as conservative as they are supposed to be, would require that borrowed funds to increase business facilities be secured not only on the buildings constructed and land bought, but also by hypothecating the patents on which this business is founded. They would thus not only be adding to the security of their capital, but they would also be setting up an accounting machinery for the objective valuation of invention and research. This is one of the most important needs of modern accounting. Some banking concerns are beginning to recognize this, and many of the wiser banks will not lend except to companies with research organi-

zations. The whole question of the place of research in the balance sheet of the company needs standardization.

It is safe to say that the question of the evaluation of research as part of a corporate structure is the greatest reason for the failure of research to be properly supported in many otherwise well-managed companies.

Research is mortgaged capital in another sense: it creates a future obligation. This is the obligation to steady growth necessary to pay off the capital attracted and to release this capital for further use. This can be done only by continuing growth in business volume and profits, which in itself is the requirement for more research.

From the broadest point of view the progress of the world towards higher standards of living, in the absence of new lands or mines to exploit, can only be brought about by wealth increase due to the discovery of new products, unless indeed we are to return to the cynical economic philosophy now called Fascism, which holds that the only way to create wealth is to steal it from some one else.

Nevertheless this does not mean that there should be an uncontrolled expansion of research, for, as every large corporation knows, there is, in expenditure for research, a very definite upper limit. This upper limit is partly fixed by the law of diminishing returns, which works sharply with research as with every other business activity. More concretely it depends on the fact that unrestrained research can soon invent so many things to manufacture and sell as to be quite beyond the capacity of an organization with limited capitalization, plant and sales outlet.

Not only must there be an upper limit to research in any company, but not all companies should undertake research, in spite of the fact that there has been a tendency for many companies who have no real excuse for a research laboratory, to follow the process unthinkingly.

Thus, before starting a research laboratory, a business executive should consider very carefully the particular needs and opportunities of his company, exactly as he would consider a plant expansion. Indeed, the considerations underlying the desirability of research for any corporation are not unlike the considerations that govern the construction of other new facilities. It is clear, for example, that a plant with a limited local market, restricted credit, or a highly specialized product, should limit its research, if any, to just that market and product. The advisability of undertaking such limited research is cluttered up with questions of policy which must be carefully studied. To do this it is necessary to look in more detail at research in its relation to business.

## Chapter III

# WHAT IS RESEARCH?

The word "research" has two rather distinct uses. The first or general use is to cover all the processes whereby an invention is conceived and carried to the point where it can be turned over to production. It is this very broad use that one has in mind when one talks of industrial research, or the research laboratory.

The second use of the word "research" is a very specific use and refers to laboratory experiments concerned with the accumulation of data needed for engineering use, or with the establishment of a principle or theory that may be needed in understanding difficulties or suggesting developments. It is this second use of the word that the college professor has in mind when he thinks of industrial research, because this usage of the word is more in accordance with the use of the word "research" in the university laboratory. There research may be defined as experimentation devoted to the establishment of data or to the test of theory.

Yet in industry this phase of research is only a rather insignificant part of the whole job of the research laboratory, —a fact which greatly militates against the usefulness of some college professors as consultants, and also against the use of unexperienced chemists and engineers in the industrial research laboratory until they have been well broken in—

until theory and practice have lived and worked together to form an understanding union.

Much more serious, however, is the confusion which is very apt to exist, within the company, between the function of the research laboratory of the company and other company activities.

The most important of these activities which tends to be confused with research are engineering development, inspection, process control, and patents. Each of these activities touches, in some way, the research laboratory. Probably, the principal cause of such so-called research failures has been the confusion of one or more of these functions with research.

It is therefore important to define industrial research a little more carefully, distinguishing it from engineering, tests, production, etc. It may be wise to give a few illustrations. Take one first from the field of refrigeration. A company is engaged in the profitable manufacture of domestic refrigerators on which it depends for its principal income. On the side it has been engaged in selling small air-conditioning units for the home. It has never made any profit on this equipment, which it continues to manufacture primarily as a matter of prestige.

There now comes to its attention a new air-conditioning device which is radically different from the device which it is merchandising at a loss. This device, for example, is heat-operated, not electrically operated, and would therefore make possible both the heating and cooling of the house with the same equipment. We will suppose that the device has reached a point where the principle has been established. First models have been built, but there is still a large amount of detailed and expensive development which must be done. This is the usual stage at which the device reaches the executive for decision. Now, if the executive having control of research

is the chief engineer, as is so often the case, his mental processes are likely to be as follows: "This gadget will cost, say, $500,000 to bring to the point where it can be sold. There is no assurance then that difficulties may not arise that will make it impractical from the standpoint of operating cost, first cost, or manufacturing difficulties. Further, there is no assurance that it can be sold in volume and at a profit when, two years from now, we shall have spent $500,000. The presumption is rather that it won't be possible to sell this device at a profit because we are not making profit on what we already sell in this field. Further, our entire staff has already all it can do in designing next year's models. Why take a chance? Let's wait until someone else tries out the field and then, when it is explored, we can go in."

Now, it is not the point of this story to argue that any of these objections are not valid. Usually they are all quite valid. Nor is it the point to show that such a conservative point of view, regarding new equipment, does not have its use in industry. Indeed, at some stage every piece of new equipment and every new process must quite properly meet such objections. The point is when? Is it when a new idea has first been brought to the attention of the company or is it just before it is ready and proven for production?

It is the function of the engineering and development department of a modern corporation to take equipment which it has been decided by management to manufacture, and to do the detailed study of the design and manufacturing process which is necessary if the device is to be produced cheaply and in volume, and if it is to be free from minor defects under field operation. Since design is never a fixed thing but must be fluid, changing from year to year as demand changes and as experience suggests and as new raw materials dictate, it is also by tradition the function of engineering to suggest and carry through, after approval

by sales, such changes in models under production as will improve these models in usefulness, appearance, price, or cost. In order to carry out this function, engineering must always be conservative and factual. That is exactly the value of engineering to the organization. If the company is to make money, nothing must go into production which has not been studied in such complete detail that there is no chance of difficulties in manufacture, sale, or use. Hence the attitude of the engineer and development man must be summed up in the following motto to be placed over the door leading from Engineering to factory: "Nothing shall pass this door which is not fully considered, fully tested, and of which there can be any possible doubt." Such a professional attitude is fatal to research, and engineering should never be given the last word in a research decision.

In this same Refrigeration company there is a research department organized under the engineering division, as is usually the case in smaller companies. Once it happened, years ago, that the research department had the idea of a cheaper domestic refrigerating unit of a somewhat radical design. The principal feature of this design was that the entire part of the machinery which had to do with making ice and cooling the cabinet was separate from the cabinet and could be manufactured and shipped independently of the cabinet. This was such an obvious advantage to any one connected with modern production line manufacturing and tests that it appealed, in a moment of weakness, as new ideas will, to the chief engineer, to the president and to the sales department executive, who at that time had the unfortunate habit of dropping in to see what the research division was doing. It seemed to be just what the company needed for the spring campaign of increasing refrigerator sales at the low price level. So the order went down from president to sales executive to chief engineer to engineer in charge of the

research department: "Get the T-unit ready for immediate production." There was no time to go through the regular design divisions,—not well separated at the time,—so the work of preparing specifications and production had to be done by the research department which had made the original design. Under the conditions, research did a remarkably fine job of getting out production drawings and specifications and cooperating with production engineering in arranging tooling and assembly procedure. I am sure that none of us from research who were even slightly connected with this rush order production job failed to get very valuable experience from the contact with the details of production engineering and product engineering. But, the unit was so full of defects that it barely survived a year's production, and instead of being the success hoped for, it probably was responsible, as much as anything, for a loss in relative position, in the industry, of this company.

This was not because the men in the research department were less competent than the men in production engineering, nor even that they were less experienced in production, though that played its part. The primary difference is in point of view. The research man, if he is to be worth anything, must be able to find the grain of gold in the pan of gravel; the development engineer must be able to see the fly in the ointment. These attitudes,—the one trained to look for what's wrong, the other to see the valuable features of a complete failure,—make engineering and research complementary to each other, but also miles apart. In this particular case so much emphasis had been put on new features of the design that defects which any trained man should see had been overlooked.

In most chemical concerns this function which we have called "Engineering" is performed by a department called "Development." In a chemical concern design of something

of which you can make blueprints is of relatively little importance. A new product such as the complex diamine of adipic acid "Nylon" isn't a thing you design in quite the same sense as you design a new refrigerator. The draftsman, the devotee of Kent's Handbook and the slide rule expert doesn't figure quite so much as does the chemist, the analyst and the physical chemist—at least so we hope. But nevertheless the adipic acid hexamethylenediamine polymer of Carothers' laboratory has to be taken out of the stage of laboratory molecular stills, to the stage of huge reactors, heat exchangers, spinners, tenters, if milady is to wear a sheer stocking. Somebody has to work out exactly what steps to use in the manufacturing process; what apparatus to use; just what temperatures, speeds, times, pressures, amounts to use in every step; just what compromises to make between cost and quality, and most of all someone had to decide whether to make the product at all.

Development men, many of them as capable and as well trained as the best research men do not like to be called engineers. But I say they are both the same in that their value and function in an organization is in that point of view which insists in making sure that the process will work and make a profit.

I give one illustration. I have in my possession a sample of plastic which means a lot to me. Years ago when on an inspection trip for the Government I happened to visit one of the large chemical research laboratories. As I wandered around I was shown a rather unpromising looking new plastic. (This was before the great vogue for these materials.) I begged a sample and, since I was a Government man, was given one. With radio insulators in mind I took it back to Washington and had it tested.

Out of hundreds of materials which we had tested for that purpose, it was the only one which met our requirements.

Feeling rather like Santa Claus I went back to the Company, told them the results of our tests, and suggested that the Company make and sell this material for this special use This they did and in due time a small business was built up in radio insulators. This was during the depression. The Company looking for economies saw this tiny business, which had no volume and which made no profits, and decided to license their process to another Company.

The sample I have is not a sample made in those early days. It was given me much later by the licensee of the original Company. The material is still being used in a small way as a radio insulator but it is also being used to the extent of millions of pounds in quite different fields.

The point of this story is not that the modern use was overlooked by the research laboratory in those old days, for most of these uses could have been foreseen. The point is that in 1932 the development men said: "This product has cost $500,000 to develop. The process, if it is to be done on a large scale, will take $1,000,000 for further development and plant. We will not sink that money on a product which is carrying us into the 'red' every day."

Development and engineering are thus, in some respects, the antitheses of research. They are the brake and control mechanism for the car of progress—without it no business could be safely driven—too much and it never starts.

Now in industry there is another corporate activity which, in the executive's mind, tends to become confused with research. This is the group of activities, differently organized in the various industries, which include inspection, process control, specification, raw product, approval, and final test.

In one sense there is nothing quite so different from research as the job of the man who looks for a defect in a casting or a finished piece, who notes whether all the steps in a process are correct, who specifies what grade of oil to

use in a part, and who analyzes a shipment of iron for carbon; yet this function of control is very commonly confused with research.

There are two reasons for this: first, that the operations of control, that is, the chemical analyses, the physical tests, and the final run-in test, usually require the same instruments that are used in the research laboratory, and thus are often performed in the same laboratory buildings.

The second reason is that, as nearly every research director has found, it is very desirable to keep the closest contact with inspection or tests because that department usually has the first and most accurate picture of the faults in existing equipment, or at least of those faults with which the research department is chiefly concerned. Kettering, with his gift for inspired over-simplification, has said that the main task of research is to find out what's wrong with what you sell.

Nearly every research director has at one time or another expressed the opinion that it is not the function of research to serve as a trouble-shooter or to correct faults of design or manufacture. In almost every research laboratory a very considerable part of the time is spent not on major projects of the inventive type which have formed the subject matter of these earlier chapters, but on minor projects: the correction of faults of existing apparatus, the improvement of processes of manufacture and tests, and the design and invention of minor improvements. These are more properly the functions of other departments, but for various good or bad reasons they are given to research, as a sort of complementary service.

Most of the reasons given why research should undertake trouble-shooting are bad, and the best organized research departments do a minimum of it. But there is one reason which is usually considered good, namely, to keep the re-

search personnel well acquainted with the deficiencies of existent equipment. This knowledge comes particularly easily from inspection and control. Hence there is a great tendency for research laboratories to undertake a rather large proportion of special tests, where they have the facilities. There is also a need for close contact with the orphaned and unpopular group whose responsibility it is to keep equipment up to standard.

There is another department in most large companies whose functions so intersect those of research that the distinctions between these departments should be clearly understood by all interested in the theory of corporate organization. This is the legal department. As much misorganized as some research departments are, there are probably few departments whose place in the organization scheme is so nondescript and poorly defined as the legal or patent department. Many corporations do not maintain a legal staff either for corporate legal work or for the preparation of patents, preferring to become clients of some independent legal firm, or firms, as patents and corporation law are seldom handled by the same group of consultants. However, where there is an active research department, the number of patents usually becomes quite great and even small companies find that it often pays to employ a permanent patent attorney. It is both an economy and, more important, a convenience, to have some one easily accessible who is fully conversant with the inventions of the company. Such a patent attorney in most cases handles the general legal business of the company too,—preparation of contracts, protection of the company in the multitudinous pitfalls of tax, corporate law, claims, infringements, libels, protection of copyrights, etc.

Usually such departments, when organized at all, are organized as part of the president's personal staff. This arrangement is not logical, but it has its advantages. It is not

logical, as probably 90% of the work of such legal departments is with patents, and therefore such a department ought to be part of the research division, as it is in some companies. Where organized under the president's office, most research heads do not object, finding direct contact with the president via his legal counselor advantageous.

The main point is that there must be, in any productive research organization, the closest cooperation between patent counsel and research executive, since the work of the departments is closely interlocked in theory and can be even more closely interlocked in practice. This matter will be taken up in detail in a later chapter.

Chapter IV

# THE PRENATAL FACTORS OF INVENTION

The modern research laboratory is a factory for turning out ideas on a mass production basis. The day of the single inventor has passed. Not in the absolute sense, however. Probably more inventors are working now than ever before. Probably more money is made and lost by the sale of inventions than ever before in history. But, relatively, the output of the private inventor is too sporadic and not well enough adapted to business needs. It is not possible to depend on his inventions for the great mass of new ideas needed to keep ahead of progress.

I illustrate with an example not taken from private industry but from one of the greatest American industries of them all, the United States Navy. During the last world war, when submarines were threatening our communications, over 100,000 inventions pertaining to naval warfare were sent to the Navy Department. About ten of these were of some importance as suggested ideas, but even these were in such form that they required adaptation and investigation before they could be used. As a matter of fact none, I think, was ever used in actual war.

The great majority of these suggestions were of the "fool" kind, but there were hundreds which would have worked if

conditions in actual warfare were as the inventor supposed. The trouble was that the inventors of these hundreds of inventions didn't know the conditions which must be met by devices used in naval war. Methods which would detect and locate a submerged log in a placid Wisconsin lake are quite inapplicable to a submarine in the North Sea. I have seen instruments proposed for detecting submarines which employed fine galvanometers. Such instruments would be absolutely useless on board ship, because galvanometers could not be read on a rolling ship.

This point is not unique with inventions suggested to the Navy. General Motors is deluged with inventions applicable perhaps to 1924 models, but totally useless for a 1943 model car.

This lesson of the relative uselessness of inventions made by those who do not know the conditions of use led to the foundation of the Naval Research Laboratory and the training of a small group of inventors in that laboratory. It is safe to say that this small group, in spite of the naive organization and direction of that laboratory, has done more to improve naval devices in the few fields in which they have been allowed to work, than all the untrained inventors in the country.

To produce inventions of value in this modern time, there must be available to the inventor a background of knowledge of need and conditions met in the industry which the individual seldom has unless he is associated with that industry.

For example, it is possible that the research organization set up by the Government in this war to improve naval and military devices may not be too successful because it depends on the principle of keeping research groups intact and farming out problems to them by a small group in Washington which itself has not had too much naval and military experience.

The best way to invent useful military and naval devices would be to bring the inventors to the Naval Research Laboratory and the corps laboratories of the Army, and use them as reserve officers on ships and in the field. It is possible that Kettering's National Council of Inventors without the aid of University Laboratories may turn out more valuable inventions in a month than the heterogeneous groups will in a year. This, however, is aside from the purpose of this chapter, which is to point out the importance of correct organization for research.

But before the correct organization of industrial research can be discussed, it is necessary to understand clearly the various steps involved in the conception, embodiment, development and production of new devices, be they new means of detecting airplanes or a new form of hairpin.

Inventions do not spring up perfect and ready for use. Their conception is never virginal and must be many times repeated. One seldom knows who the real father is. The period of gestation is long, with many false pains and strange forebirths. The number of abnormal creatures which see the light of day is ghastly. Few of the children of the mind ever survive, and those only after many operations and much plastic surgery.

Because of the high incidence of bastardy among inventions there is a high proportion of exceptions to all statements which can be made about the birth of useful ideas. Roughly speaking, the process of the development of an idea from its point of conception to its use in production is as follows:

New ideas do not arise spontaneously, but almost exclusively out of mental conflict between two sets of ideas. What you know is not just a list of facts. It is a list of facts connected by some sort of a mnemonic scheme which may be a full-blown theory or just an association pattern. When a

new fact comes in we attempt to fit it into this scheme. If it fits, well and good; we forget it. If not, we feel uncomfortable, like the robin who swallowed a string in place of a worm and, if the opportunity arises, we try to do something. Either we throw the fact away, which is a natural tendency, or we modify our scheme so that the fact will fit.

It is out of this enforced modification of mental patterns that new inventions arise. Suppose you have a monkey in a cage and you put a banana outside the bars. He will, if hungry, reach out his arm and get it. His behavior pattern includes the form, "Stretched arm can get banana." We now put the banana out of reach. He may, if he is hungry, try in vain to get the banana, but not if he is a really intelligent monkey. If he is an intelligent monkey, he will shake the bars and scream. His behavior pattern includes the form, "If I scream, the keeper will give me the banana." Suppose now, instead of doing that, the keeper puts a stick into the cage. This is a new situation which the monkey has not met. He must do something, because he is hungry. He picks up the stick and says to himself (or rather acts as if he had said to himself), "Ah, a longer arm!" and with the stick reaches out and pulls in the banana.

This is invention at its best. Let us, however, without depreciating the value of monkeys as inventors, take up some cases where perhaps even less intelligence is shown.

In the first World War it was discovered that the German airplane engines were producing more power per pound than the American and British engines. This was a situation where need for action was evident and emphatic, therefore one of the requirements for invention was present,—the need. The problem was then turned over to British and American engineers to find out why the German engines were better, and produce their equal or superior. I do not know how many various research groups were involved but, I

dare say, nearly every one proceeded in about the same way. They acquired samples of the competing machines, set them up, tested them on the test block, and were astonished to find on test that the engine with American gasoline failed to perform as they had been led to expect. They did not produce greater power and they developed what was then known as "spark knock." At this point the engineer-minded automobile expert sighed with disgust and said, "To hell with it. This is just another case where the airplane pilot is making an alibi for his poor showing in combat by reporting that his enemy has a better machine."

In America, where the engineers working on this problem were mostly from the young automobile industry, this attitude was particularly understandable. Most of them had had experience with automobile salesmen who falsely reported that they had a better product, only to have it show up very badly in test.

But there were some engineers who went a little further. After having found the poor performance of the German engine, they took it apart and compared it in detail with American design. It was then immediately evident why the German engine was poor. It had a much higher compression ratio than was considered good practice. That is why it had such a pronounced spark knock.

At this point they sat down and wrote a fine report showing that the German engine was inherently of poor design, and exactly why.

This is the mental situation. The poor performance of German engines, with compression ratios of six, fitted exactly into the mental pattern of the engineer. His theory being that only engines, with compression ratios of less than four, could be safely used for airplanes. Therefore there was no incentive for invention, hence no invention, only a nice conclusive negative report.

But there was one group of inventors that had a different pattern in regard to engine knock. Headed by C. F. Kettering, they had met "knock" before in connection with the development of the self-starter for automobiles. At that time the knock was supposed to have been connected with the method of ignition, with compression, and with the amount of carbon present. Their experience had been very painful. When the self-starter first came on the market a few years before, all the troubles which every designer had, due to knock, he had blamed on the self-starter. Hence, Kettering in order to convince customers that the knock in their engine was not affected by ignition had had to investigate "knock."

Now, Mr. Kettering does not have a nice orderly mind that loves theory. He is one of those people who pays attention to anomalies of performance, not to regularities. Hence, he isn't a scientist at all, but he had noted that knock was a most erratic and fugitive phenomena, depending on a great many things; not on carbon, ignition, or compression only.

When he found that the engine knocked, and still was reported satisfactory in the German plane, he was willing to consider that maybe the engine might work under German conditions. So he wrote for a sample of German gasoline. When it came he found that with the German gasoline the engine did not knock.

Here was another case where a situation, namely, that German gasoline could be used at a higher compression ratio than American gasoline, had to be fitted into his mental pattern. So the question was asked, why?

The answer depended on his mental pattern and on that of his associates. It happened that neither Kettering nor Midgley were chemists, so the body of theories they had in stock was very inadequate for the task. A chemist would immediately have analyzed the gasoline, and in time would

have found it to be a different class of gasoline than American gasoline in that it contained a higher proportion of aromatics and less straight-chain hydrocarbons. In due time, say 20 years or so, this would have led to a solution of the problem of making better gasoline, as indeed this same observation has done. But neither "Boss Ket." nor "Midg." were chemists. They simply looked at the German gasoline and saw that it was dark brown in color. Therefore, it occurred to them that maybe the color somehow absorbed the ultra-violet light from the spark and thus prevented premature explosion, which they thought caused knock. They had thus hit on the theory. An obvious way to test it was to send for a dye which would color American gasoline. So they sent to the stockkeeper and asked, "Have you a dye soluble in gasoline?" The stockkeeper didn't have such a dye. Being imbued with the stockroom keeper's instinct, Mr. Chase asked if something else wouldn't do. "What have you got that will dissolve in gasoline and color it?" Iodine was the only thing Chase could think of. So they took iodine, and it worked!! The American gasoline with iodine in it didn't knock in the German engine. By this time Kettering and Midgley were feeling very proud of themselves. They had invented a theory for knock and proven it all in one afternoon, namely, that knock was due to ultra-violet radiation. The trouble is that just then the messenger sent downtown after a soluble dye came back with several such dyes, which were tried, and not one of them worked!! The discovery of iodine was just a lucky accident, and the color of gasoline had nothing to do with its knocking. The red color of ethyl gasoline is a means of identification, and has nothing to do with anti-knock properties.

Now there was nothing that could stop the eventual discovery of anti-knock agents. Even if the theory they originally had was wrong, it had led, by the lucky accident of the

use of iodine to one very potent fact, that the addition of small quantities of something to gasoline could stop the knock in the engine. The rest of the task was simply to find a more suitable material than iodine. Iodine would ruin the engine.

Now if either Kettering or Midgley had been organic chemists, no doubt they would have followed the path of analysis again and eventually wasted a long time trying to find out what in the German gas was responsible for the prevention of the knock. Not knowing organic chemistry, they elected to try, still further, the Edisonian method, try every bottle on the shelf. Soon they found that selenium oxychloride would stop the knock very effectively as would tellurium compounds. But again it was unsuitable for practical use because of the horrible odor of the engine exhaust. At this point the supply of bottles on the shelf ran out, and they were faced with the necessity of building, in the laboratory, new compounds to test. Again we have the requirement for doing something about it which is one of the three elements of invention.

They then reviewed the situation (the second element). Iodine had worked. Tellurium and selenium had worked in several combinations. Every other element they had used had failed. Now what did their background of experience suggest? Again they called on their primitive knowledge of chemistry and got out the periodic table of elements to see what was common to iodine, selenium and tellurium. It was quickly evident they belonged in the lower right hand corner of that table. They then decided to test the remaining elements in that section of the table, lead and bismuth. At this point the purely mental phase of invention was done. The actual test of various soluble lead compounds, including tetraethyl lead, was just detail. So was born ethyl gasoline, and so are born all industrial inventions—need plus situation, plus idea or theory.

Let us consider another case chosen because it represents a different ratio of the three components—the discovery by Langmuir of the process of hydrogen welding. This is a case where the invention sprang from what is called "pure" research. Langmuir was hired by General Electric primarily because the tungsten lamp, satisfactory as it was, tended in course of time to have the same trouble as the old carbon lamp, i.e lose brilliancy because the inside of the globe became coated with a black deposit. Recognizing Dr. Langmuir's particular talents (which is the essence of management), Dr. Whitney gave Langmuir unusual freedom in the choice of method. This was inevitable, as the problem was one which seems very clear cut, yet had yielded no results in the hands of other workers. Perhaps he, with a point of view different from others in the organization, would have a different approach if left alone and not tied down too much.

Dr. Langmuir has written an account of the devious path taken in the partial solution of the problem of blackening, but it would take too long to give the details here. We are concerned with an invention made incidental to the main purport of Dr. Langmuir's research; an invention to which he gives only a few lines in his account. This was the discovery of the hydrogen welding torch.

We give this example because it was a discovery which did not arise out of any conscious demand for a better welding torch or out of any special interest of Dr. Langmuir in welding; a field to which, as far as I know, he has never contributed anything before or since.

The invention came in this way. In connection with his investigation of the blackening of tungsten lamps, Langmuir was systematically studying the effect of small quantities of different gases in the lamp bulb. This was partly because it was realized that perhaps traces of these gases might be mainly responsible for the blackening of the bulb and for

other effects on lamp life which Langmuir was beginning to understand.

Among the gases studied was hydrogen. Hydrogen, when introduced in traces in a lamp, behaved peculiarly. Other gases such as nitrogen and argon would conduct heat from the filaments to the walls of the lamp in a quite regular manner, according to theory. Hydrogen carried heat from the filament like other gases at low temperatures, but at high temperatures, it carried much more than was expected. At the same time a definite amount of hydrogen disappeared from the bulb and was absorbed on the glass walls. This suggested to Langmuir that something was happening to the hydrogen at the high lamp temperatures; something that absorbed lots of heat and changed the hydrogen so that it would stick on the cold wall of the bulb. He then remembered that R. W. Wood of Johns Hopkins University had found that, under certain other conditions, hydrogen dissociated, and formed a new gas, monatomic hydrogen. Like most chemical processes of dissociation, this process absorbed heat which accounted for the large heat loss of the lamp filament when heated in hydrogen.

Even here, there was no suggestion for invention, because though there was a new fact, there was no conflict. The fact fitted nicely in Dr. Langmuir's background and a nicely fitting fact is an anesthetic to mental effort. But, Dr. Langmuir did have one incentive for action. He was still trying to understand exactly what went on in the lamp in the presence of a gas, and to do that it was necessary to translate his guess, that hydrogen dissociated on the tungsten filament, in terms of numbers. No fact is worth much unless it can be expressed accurately. So, purely to check his hunch, Dr. Langmuir calculated from his experiments the heat of dissociation of hydrogen and found it to be about 90,000 calories. This was about the value that others had estimated

about this same time, but somewhere in Dr. Langmuir's mind the fact struck "pay dirt," as it had not done in the minds of others. The reason was that Dr. Langmuir had learned, in laboratory conferences and by his own experiences, that there was difficulty in brazing tungsten with pure copper. It occurred to him that if hydrogen could dissociate in the electric discharge as Wood had found and if it could dissociate by heat as he had found, it ought to dissociate in a welding arc. Then outside the arc it should recombine, giving off this enormous quantity of heat which could be used for welding metals in the absence of oxygen. It probably didn't take an hour to test this. Thus was born the method of hydrogen welding.

Wood and the spectroscopists who also knew that the recombination of hydrogen gave out heat, had the same facts as Langmuir. They didn't have, in addition, the facts which made them see any commercial need or use.

The best proof that invention springs out of a conflict between an external fact and an internal fact comes from inventions which were not made even though the observer had most of the elements. Such failures to see an invention are seldom published for obvious reasons. I, thus, am compelled to take cases from my own experience, of which I am not very proud. Back in 1912–13, Sir William Ramsey, the discoverer of argon, neon, krypton and xenon, lectured before the Lowell Institute of Boston. I helped in the preparation of the experimental demonstrations for these lectures. Sir William, a brilliant lecturer, gave three or four lectures telling of his discovery of these inert and very inactive gases, illustrating with experiments. He told of naming these gases neon, which means new, argon, which means lazy, krypton, which means hidden, and xenon, which means strange. He told of his attempts to make these gases combine with other substances and very vividly pointed out that because they

were so inert and reluctant to combine, they were useless scientific curiosities. Then he said in his last lecture, "Some of you will ask how we can be sure that these gases are really pure substances; not just mixtures. I will show you." He showed a series of glass tubes set up so that an electrical charge could be passed through them. "All pure substances," he said, "are characterized by the fact that they give out, under an electrical discharge, their own special kind of light." He turned on the current. The five tubes lighted up with a pale glow of a distinctly different color. "Under different conditions of discharge," he said, "these colors can be intensified." He then switched a condenser in the line, and the tube containing neon flashed up a brilliant orange red light. It was very striking. We all applauded and went home. Not one of the five hundred or so who heard the lecture realized that we had seen the first Neon sign. It was only some years later that Claude, seeing exactly the same experiment, realized its commercial importance. Why? Because, Claude was engaged in the purification of liquid air from which argon, neon, etc. are waste by-products. He was looking, therefore, for a use for them, so as to cheapen his process. If anyone looked at a Geissler tube, containing neon, from the point of view of fitting a use for the gases, he could not miss thinking what a beautiful advertising sign these tubes would make.

Another case: I was once engaged in looking for a liquid which could be used to dry air. I had studied physical chemistry and knew the principles of air conditioning, so it did not take more than the reference to a table of vapor pressures to suggest that a strong solution of lithium chloride might be useful, as it turned out to be. I was then working for the Frigidaire Corporation and through them I applied for a patent. In this patent we claimed the use of lithium salt solutions as drying agents for air. It was only some

years later, after I had become more familiar with absorption refrigeration, that it occurred to me that lithium chloride could also be used to absorb water when there wasn't any air, thus producing refrigeration. I had the facts again, but not the experience into which it was necessary to fit them. This lapse prevented Frigidaire from getting a broad patent on the use of lithium salts, which would have generally protected what is now one of the most important applications of these salts.

Invention, thus, is not an esoteric art, practiced by a genius in some attic on somebody's cookstove—at least, not usually. Under modern conditions, it is a process of production, which is just about as controllable as any other production process of this machine age. Give a man a background of diverse education and experience, which has been well digested so as to form a satisfying, well organized picture, and give a certain practice and facility such as is obtained in university laboratories. Then give him certain new facts or needs, which do not fit into his mental picture recipe and require him to do something about it. With this, inventions just pop out with regularity and astounding fecundity. They can't help it. The mind is like an amoeba; if confronted with a foreign body, it must find a way to digest it, go around it, or it must die.

## Chapter V

# THE POSTNATAL FACTORS OF INVENTION

So much for the process of invention as a mental act. But, this is not all there is to the process of invention in the practical sense. After the conception of an idea in the "womb" of a fertile mind, must come a long and painful process of gestation which, in more-cases than not, is fatal to the foetal idea and sometimes to the parent.

Disregarding the frequent miscarriages, the development of an idea into a useful process or machine follows a rather well defined course. Having conceived, the first thing the wise inventor does is to record that fact, giving in full detail the parentage and having everything properly witnessed. This apparently obvious piece of routine is, in an astonishingly large number of cases neglected with the result that having had all of the pain and expense in bringing birth to a new device, the inventor too often finds himself deprived of his mental child, perhaps even before it is publicly born.

It used to be thought that the best way to protect mental offspring during the period of gestation, was to keep the approaching birth secret. This presumably was on the theory that the idea in its foetal stage might be stolen. It is now realized in all except the most backward companies that though a moderate amount of discretion may be advisable during the gestation period, it is of vital importance for the

unborn mental child to disclose its existence not only to those within the organization, but also, under guarded conditions, to others not directly connected with the organization. Under the present patent laws, such disclosure is a vital step in proof of invention.

The real danger of loss of an invention to someone else, it is coming to be realized, is not theft of idea, but due to the fact that in an astonishingly large proportion of cases the same idea is conceived in different laboratories at the same time. Many important patents have to fight ten or even fifty interferences among inventors of the same idea, who have filed patents usually within a year of the earliest or senior party.

This is not due to some sort of mysterious thought transference between inventors. It is simply because inventions under modern conditions arise through needs, and as needs arise all through a given industry at about the same time, and as the invention to fill a need is usually more or less evident once the need is defined, it is not a matter of accident that the same idea will arise simultaneously in various places. The only way for the earliest of these inventors to protect his idea is to keep careful records and disclose his inventions to others as early and as fully as possible.

Having thus put his invention in written form and disclosed it to others, even though it be only a very crude and not fully formulated hunch, the next step is to stop and consider; to take time to think the thing over and to learn all that there is to be learned about it; what others have done in this field and to discover the laws and limitations of the proposed process.

In this stage, in well organized laboratories, the inventor calls for the help of the patent department, the search staff of the scientific library, the consultants and anyone else he can get to make suggestions. Now is the time to look at the

idea from all angles, in order that the idea be formulated as broadly as possible for the sake of broad patent protection. Now is the time to formulate the limitation of the idea. Now is the time to plan the program for further investigation and now is the time to kill off those aberrant and truant ideas which seem, even to the parent, impractical or useless. It is the parent himself who should practice infanticide at this stage. Others will have the opportunity later.

It should be realized that research is an expensive process, because highly paid men are the laborers in the early stages of development of an idea. Economy of time is as important in research as anywhere. But, on the other hand, a few hours work at this time may bring very large returns and in this early stage, only the inventor himself has a ghost of a chance of judging the value of the time.

Now having studied the possibilities of the invention as far as can be done by pure study or simple laboratory work done by the inventor himself, the next step in orderly progress is the dreaded step in every inventors life: getting executive approval for further development work.

So far, the nurture of the idea has been entirely under the control of the inventor. Now time and money must be spent, other than the inventor's own time and that of the library and patent staff. The baby's future must be passed upon by the inventor's superiors and a preliminary decision made as to whether or not to keep the infant alive or let it perish stillborn. In wisely organized companies, this decision will never be made in private, by a company official or committee. It will always be made in cooperation with the inventor, giving, as is unfortunately not always the case, every consideration possible to the inventor's desires in the matter, for there is usually far greater danger in killing a man's pet idea too soon than there is of letting it go on just a little further.

In some cases, however, especially in a small company, it

is necessary to pass on a proposed problem requiring time and money very early, because with their limited staffs and research appropriations, it is necessary to keep very close, in all projects, to the reality of the company's business policy. In a company manufacturing adhesives, it might not be advisable to work on a new detergent, even if it were a natural outgrowth of the laboratory's previous work and even though it were of itself valuable, not because of the development cost but principally because the company is not in a capital and market position to undertake this foreign activity.

This is a very unfortunate fact, for it is rather surprisingly frequent that in small specialty companies many a valuable idea must be killed which in larger companies would prove of real economic value. There is nothing that can be done about it, unless in special, very rare cases, it is possible to sell the raw idea to one of the larger companies. However, few companies have the connections or the courage to do this.

Suppose now, by great good fortune, the new born infant idea is granted a lease of life. Human infants are usually allowed a period of rather careful nurture before they are exposed to the rigors of the outer world. Not so the infant idea. It must fight for itself, from the moment of birth, for the usual attitude of those who assisted at its birth is, "Oh well, it's a poor thing and probably won't amount to much, but let's let it live for a couple of months or so out of kindness to Tom or Bill, whoever the parent was." So the new baby is granted five hundred dollars and two months time to prove itself. As a result, the first steps in experiment with the new idea are usually limited to the construction of a very crude working model or to very simple tests.

There are reasons why this should be so, besides the over penuriousness of budget authorities. In the development of new devices or new processes, it is much easier to get further

support for a device for which a working model exists. Then too, our patent law is so drawn that the real date on which the patent office and court rely for establishing the ownership of an invention, is the date of reduction to practice, i.e. the date of construction of a model that worked. This isn't exactly true in theory, but in the practice of law it is very nearly true.

The inventor, let us now suppose, has spent his original allotment and possibly one or more extensions and has produced a working model. Now, in proper organizations, comes the decision that really counts. The device is made the subject of a report summarizing as fully as possible what is known of the device, its limitations and its potentialities, as far as the original inventor is able to see them.

This report goes to the division chief, or perhaps, even higher, if the device is of unusual importance. With this report goes a carefully formulated plan for the next steps.

Here is where the infant first finds the going tough. For here is the time at which a death sentence should be pronounced, if it is justified, on the basis of evidence then at hand.

Again I give a personal experience, because few would like matters of this sort to be published. I had at one time invented a household boiler, rather an odd boiler, but still a boiler capable of heating a house in winter and cooling it in summer. I had built a very simple form of this device. I thought it a good idea (I still do for that matter), so I submitted this to the small company by which I was employed and disclosed it to them. The company was at that time engaged in marketing another air conditioning device, so it was a natural thing for them to develop mine. I was not present at the discussion of this device, as I should have been, hence I do not know the reasons why it was turned down, which I suspect may have been different from those set down

here. But it should have been turned down because it was a device which would cost a great deal to develop—more than the company could afford. It would be unwise for the company just beginning in the air conditioning business to start another attack on the field, until the first had begun to pay its way.

Any practiced inventor or research director could give case after case showing (a) the importance of stopping unfruitful or useless research projects as soon as possible, and (b) the mistake made of stopping an invention too soon. It is at this company conference, which decides whether to go on with a project or drop it, that the success or failure of research organization and direction shows itself. Almost any research laboratory will produce vastly more inventions than it can possibly develop. Invariably, the important question is which ones to drop.

We have now weeded out probably 95% of the inventions which come to the research directors' attention. Let us suppose the present invention belongs to the elite. From now on the going is, as should be, easier.

The invention now being reduced to practice, in many cases, is in shape for starting the formal legal procedure preliminary to filing a patent. This procedure includes formal search, and formulation of specifications and claims. It is often wise to do this even if it is known that the application may never be filed on the invention as it then stands. This is advisable, if for no other reason than the instruction of the inventor who, as any patent attorney will tell you, seldom knows enough about what he has invented! That is to say, he knows not what is an essential element of novelty and value which is patentably distinguishable from what has been done before.

It seems foolish to say that the inventor doesn't often know what he has invented but consider again the invention

of lithium chloride for drying air. That is what I thought I had invented, and that is what I was able to persuade the patent attorney and the patent office that I had invented. What I had really invented was an absorbent for water vapor. Of course, I should have known that. If anyone had asked me even then I would have agreed, but no one did, so we lost about half the value of an invention.

But the importance of this next step in development after formal reduction to practice is not this legal phase. It is in the research, which in almost every case follows first reduction to practice. The word research here is for the first time used in its limited but exact sense.

Some might suppose that having built a model of a device that works, that that would be all there is to it. Most books on invention rather foster that delusion. Of course it is sometimes true after you have invented a hairpin and made one. That is all there is to it, except make more just like the first one, and sell them.

I understand this was the case with the first hairpin, but even here I have my doubts. In the usual case the first working model is only the beginning of the invention. There are several reasons for this. Even if the first model is perfect, which it never is, it is always conceivable (in engineering if not in grammar) that there might be a better one possible.

Hence, after invention must come a whole series of laboratory investigations, studies of the effect of variation of material and design on the product. This involves tests on the completed model and also a test on every part, or if it is a process, every step separately. The variations studied are materials, conditions of manufacture, and design. The purpose is to learn enough about the baby, so that its design can be improved and the defects removed so that upkeep can be lowered and, most of all, so that the cost of reproduction can be reduced.

In short, the stage in development which comes after the first successful model, is characterized by a laboratory search for information which the research group needs to recommend a final design, and which the production group needs to turn that research design into a design that can be manufactured in a factory at a low cost.

In a sense, the "re" in research is justified. Research in the strict sense in which it is used in academic laboratories, and as it is practiced in this phase of invention, is simply taking over again in an accurate and controlled manner the mental steps—hunches though they have been, which the inventor took in his first rough design. There may be occasions where search is necessary at this stage of development, but more usually the real activity is precise formulation in accurate experimental terms, of that which has been formulated as a hunch or hypothesis.

Dr. Langmuir was doing *re*search when he reinvestigated the loss of heat of the tungsten filament, in order to verify his hunch that the excess loss at high temperature was due to dissociation. It was research because he had already observed the phenomenon he was to study anew, and because it was done to verify a hypothesis. It is also research to discover that the ratio of the valve clearance, in a compressor using dichlorodifluoromethane, must be three times that of an ammonia compressor. It was research not because experiment was unnecessary, but because it was done only after tests had shown that there was something wrong, and after an hypothesis had been made that the failure of the machines to perform was due to value restriction and was not due to any one of a million other things.

Hence, research, in the correct sense, is an investigation, undertaken by plan, for the sake of verification of an hunch or theory. Even this is not all of the process between idea and something to sell.

At this stage, research having been completed, that is, after the research laboratory has completed its investigation and knows how the new device works, what it will do, how to make it, what materials to use, and how much it will cost, the next step is to get the company to manufacture it. Again this would seem to be an unnecessary step. If the thing is good, works, can be built for a price, and has advantages over other gadgets on the market, of course, all the laboratory director has to do is to call in a conference of the chief engineer, the production manager, and the vice president in charge of sales, and say "Here it is gentlemen. It's yours, go ahead and make it."

Once I though so too but, unfortunately, I've learned ater. Most research directors that I have seen are rather mild-mannered men not given either to profanity or strong drink. But it can be guaranteed that this is one subject, the mention of which will bring a strong reaction from any of them.

Dr. Kettering put it very mildly when he said in a recent book—

> "The greatest durability contest in the world is getting a new idea into any factory. It is well if the management understands this and will constitute itself the sales department for the research organization. Otherwise, the hard-boiled men in the factory will put research men out of business in a fortnight. The most pitiable thing in the world is a man who does not know how terrible factory organizations are. Putting a research man up against it is like throwing Daniel to the lions when they are all hungry. The research man after finding his problem starts to gather up the information, by and by he will report: 'this is what I have found out about the thing' and then when you read the report to

the factory heads, they say, 'Applesauce' and into the waste basket goes the report. When we present a new idea to people, their first instinctive reaction is against it. Nobody likes a change. That is the one great thing you must understand in the psychology of research.

"The human family in industry is always looking for a park bench along the road of progress where it can sit down and rest but the only park benches I know of are immediately in front of the undertaker's shop."

"Profitable Practice of Industrial Research" Malcolm Ross, Editor.

He has also put it, with embellishments, that I shall not add, that the surest way to sell a new device to his company, General Motors, is to sell it first to a competitor. These two statements of his, nicely complement each other and suggest both the cause and cure of the strange reluctance that those concerned with manufacturing have in accepting new ideas, especially from their own organization.

To manufacture a new product means loss of equipment, cost of training men in new processes, shut down, interference with schedule, high cost per unit, high rejection cost, loss of profits, and expenses for new machinery. Many a student of economics has chided Henry Ford for holding on to Model T for year after year, but not any one who knows the cost of the annual change of model would thus criticize him.

The research director's job, therefore, is not done when the product has been invented, designed, and proven in theory. He has to sell it, just as much as if he were a private inventor. There is nothing harder to sell than an idea, and he has to sell it to a group, the majority of whom are instinctively hostile to any new product. A new product to the president, sales manager, chief engineer, and production

man means new demands on capital and earnings, new problems of design, and interference with the normal redesign of the profit lines of the company. It means new problems of production, griefs in factory and field, new problems of sales, and distribution.

And to make it worse, the average research director who never sold a penny's worth of anything in his life, does not make a good impression in a business conference, especially when he does not have the facts of the business aspects of his invention at hand. It is a wonder to those of us who have tried to sell ideas how some research directors succeed. Usually it is by mere force of personality rather than by legitimate sales methods.

There is no part of research more important than sales, and this means, in order of increasing importance, a good article, proper preparation of sales presentation, full knowledge of the financial situation of your customer, knowledge of the peculiarities of the personality of those to whom you sell, and most of all proper *personal contacts.*

Chapter VI

# RESEARCH FOR THE SMALL COMPANY

Up to this point the book has had to do with what might be called the philosophy or theory of research. Like all theories its value is best tested by applying it to the detailed problems which arise in practice, in this case the business decisions involved in the foundation and operation of a research laboratory. In doing this I will have to repeat much of what has been said, but from a different point of view.

The first of these decisions is whether to attempt any research at all; the next whether to hire the research facilities, outside the company; or whether to build a laboratory of your own. These decisions have particular point for the smaller companies, and it is to these that this chapter is especially directed.

It has already been indicated that under certain rather clearly defined conditions, research should not be undertaken either directly by the company or indirectly by consulting groups.

The first class of company which should not undertake research on its own is that with restricted distribution. This includes retail stores, companies performing local services, manufacturing companies with local distribution, and manufacturing companies with restricted markets. Most of these companies do not have the capital or experience to go adventuring into the unknown fields of business, which

research always opens up. The prime reason why these companies, as a class, should not engage in research (there are some exceptions) is that if a successful new idea is invented, it is impossible, or highly disadvantageous to attempt to restrict the sale of that article to a particular locality. The products of research are practically never products for a particular local demand, but are products which require national or even international distribution. The appeal of the new is not restricted by political boundaries.

Now, to be sure, there are methods where a product developed for a local milk company, say, can be protected and yet distributed widely. These methods are patenting and licensing to a national manufacturer to be distributed through the regular trade channels. Sometimes local companies show surprising ability to expand because of trade advantage due to the possession of a new thing; but, in general, it will be hardly ever found advisable to set up a permanent research organization on that small chance. What every company should do, regardless of whether it has proper outlets or not, is to encourage its ordinary staff to submit new ideas, and encourage the development by every way short of a permanent organization. Many a useful invention dies an untimely death in some dry goods store simply because the management does not have the vision to assist inventors on their staff with legal and engineering advice, business contacts, or even financial aid. This does not take organization, just human understanding and vision.

Though small local companies and selling organizations can seldom find a proper justification for undertaking reresearch on their own responsibility, they are not any the less in need of research. Quite the opposite—a small company, a local drug store, or a gas company is in a particularly vulnerable position against the attack of chain stores or large electric utilities, since these chains are able, with their large

markets, to develop exclusive goods or services which the little fellow cannot match. Undoubtedly much of the loss of relative position by the local stores as compared with the national stores and mail order houses is due to the fact these stores not only can sell more cheaply because of their greater buying and selling volume, but also because of the fact that their goods are more modern in design, better in quality, and often include exclusive items developed by their own research or by exclusive arrangements with manufacturers and research laboratories.

To meet this, the local company has but one recourse, and that is through the organization of trade association laboratories. There can be no doubt, that it is true, that the organization of a strong trade association with research laboratories and test laboratories, to maintain quality, gives the best chance for the small independent grocer or drug store to retain or regain his business.

There is a tendency in Washington to look on the trade association as a device in restraint of trade, and sometimes it has certainly been that. But trade associations for research, test, and under limited conditions for mass purchase, are the real salvation of the small independent business man.

So far, the discussion has been confined to companies, that could never, for one reason or another, recover any reasonable part of the money that would be necessary to form a research laboratory and support it during its unproductive years.

We now turn to an intermediate class of companies—those with a national market but still so small and restricted in one way or another that the expense and gamble of research must be considered with particular detail. A large company doing a $50,000,000.00 business can afford to spend 1% or $500,000.00 for research, without upsetting in any way the business structure, if returns are delayed or involve unex-

pected difficulties. But, consider a small company, say a chemical company manufacturing crayons, or a machine shop manufacturing a special line of ball bearings. With a sales of one-half million, one per cent, represents five thousand dollars, for which you can buy almost no research at all. Yet, such companies have no less need of research than a larger company. Often more. Working in a very specialized field, the method of the trade association is impossible, because research value in a given line of business is competitive. One wants a better method of producing bearings, or better crayons, principally to get an increased share of the total business in bearings or crayons. It is all right to share the results of your research with someone who has a different market, as with a grocer in a different town but to share the advances of method or product with one who competes is quite a different matter.

There are two ways of meeting this situation, which have actually been used. One, the method which depends on use of consulting industrial research laboratories, or such endowed cooperative institutions as the Battelle Memorial or Mellon Institutes. In practice, this method consists in hiring research done on some particular problem on a cash basis. This isn't the cheapest way to operate, or the most efficient, but it has the advantage of providing terminal facilities. The work can be stopped at any time that the idea does not seem profitable and there need be no commitment to continue. Let us follow this with an example. Someone in the ball bearing company gets the idea that after getting the balls round to a half thousandth, he can case harden, using one of the gas carbonizing processes, without distortion, and then polish the final balls, producing a highly superior and cheaper ball. One procedure is to put the whole problem before some manufacturer of carbonizing equipment, who will often be willing to go to surprising lengths to do free development work and

to adapt its process to a new product. But, to do this means that the ball company will soon find his competitors using the new process. The other way is to save, say $25,000, start a fellowship at Mellon Institute, or engage a competent consulting laboratory to do the job. In this way there is no need of building a regular research organization.

It is surprising how often this procedure leads to results of great value, in spite of the fact that, in general, research has the greatest chance of success when closely geared to the company plant operations. Mellon Institute, as an example, publishes, from time to time, a summary of the researches which have been completed, with some information leading to an evaluation of the economic value of the results which have been obtained. Among others, an important industrial process developed at this institution is the Tenderay process of tenderizing meat, which was supported by Krogers Consumers. This has brought to the average housewife aged beef of better quality and flavor than was available a few years ago.

Indeed, where the problem is well defined and where it is of the type which is not continuing, there is a considerable value, even for companies with permanent research facilities, to farm out to the industrial laboratories and to such university laboratories, as offer this class of service, such problems as do not require close operating contact with the factory; as do not involve special skills or training or apparatus not available in the regular organization; and such problems where it seems that the purchase of these special skills and equipment could not be justified on a continuing basis. For example, Dow Chemical Company, investigating a future market for bromine compounds, desired to investigate the possible use of bromides as an absorbent for water in domestic and air conditioning apparatus of the absorption refrigeration type. This project could be justified only to the point of

developing a new process in which a customer might use bromides and thus develop a market. It could not be justified to the point of Dow Company going into the manufacture and sale of air conditioning equipment. There was, therefore, no sense in hiring, on the permanent staff, experts in air conditioning and absorption refrigeration, nor was there a point in permanently giving space and equipment in the general laboratory for this "market research." The Dow Company has, therefore, set up a consultant laboratory, for the purpose of doing research looking toward an increased market for its goods and also for doing work for customers who use Dow products. This consulting division uses as its facilities university laboratories under a contractual arrangement. Similar results could be obtained using standard consulting laboratories and in some cases using consultants at one's own laboratory.

But, it is to the small manufacturing company, with a limited production and a great idea, that the consultant laboratory offers its greatest field of usefulness. To prove this, one can list some of the current researches sponsored from the latest reports of the Mellon Institute.

| *Project* | *Sponsor* |
|---|---|
| Electro Deposition | Standard Steel Spring Co. |
| Iodine | Iodine Educational Bureau |
| Meter Technology | Pittsburgh Equitable Meter Co. |
| Pearls | Pearl Associates |
| Anthracite | Anthracite Industries |
| Calgonite | Calgon Inc. |
| Chains | McKay Co. |
| Commodity Standards | Kaufmann Department Stores |
| Heat Insulation | Phillip Carey Co. |
| Pressing Machinery | U. S. Hoffman Machinery Corp |
| Protective Coatings | Stoner Mudge Inc. |
| Garspar | N. S. Garbisch |
| Urban Air Pollution | Dept. Public Health, Pittsburgh |

| *Project* | *Sponsor* |
|---|---|
| Precast Concrete | Cemenstone Corp. |
| Graphite | Vesuvius Crucible Co. |
| Metal Working | Wm. B. Scarfe & Sons |
| Oil Cleaners | The Fram Corp. |
| Mercerization | Aberfoyle Manufacturing Co. |
| Protective Metals | H. H. Robertson Co. |
| Powder Metallurgy | Plastic Metals Inc. |
| Safety Fuses | Ensign Bickford Co. |
| Lignin | Marathon Paper Mills |
| Petrolatum | Chesebrough Manufacturing Co. |

The second way for the small manufacturing company is to start a small research laboratory of its own. Case after case can be given where this procedure is successful. Before doing this the company directors must understand the risks that are being taken. I can show the dangers and pitfalls as well as the rewards of the small laboratory best by some illustrations:

Take first the case of a company manufacturing gas appliances, a field which is much overcrowded with small foundries. To make a success in this field there are three possible routes: First, manufacture a cheap product and sell it locally (high freight rates would make it impossible to compete on a price basis far from home). Second, build up a national name through advertising and thus dominate the field. No one has succeeded in doing this in the field of gas appliances as General Electric has done for electrical appliances. Third, study the field, determine which portions are not adequately exploited, develop new uses for gas, and by research develop apparatus suitable for these new uses.

The business of this company was originally based on the invention of a new type gas burner for certain industrial uses. This field, however, never proved very profitable. Later the Company developed the first successful automatic

burner for converting domestic coal-fired furnaces to gas-fired furnaces. For some time they had the field to themselves and it proved to be very profitable indeed. Then, when competition developed, as it is sure to do, in the midst of the depression, they saw an opportunity to improve the burning of gas in the field of industrial furnaces and lehrs. They therefore began to manufacture these very special furnaces. When this field became partly exhausted they developed various heat treating processes using gas. Such processes were carburizing, decarburizing, and the pickling of metals. More recently, they have developed a process of radiant heating which is giving severe competition to electric heating for industrial purposes. Their latest achievement is the commercialization of an air conditioning process using gas.

A business such as this can be profitable only if it has: First, a management thoroughly embued with the philosophy of research; second, a research organization so closely associated with sales and production that there is no line of distinction; third, that sporting spirit which dares year after year, in the face of great odds, to venture the whole future of the company on a single throw of the research dice.

I remember visiting a small company which has been in existence for over three generations and is still making a success in the field of bulk chemicals. This is the field dominated by such titans as duPont, Carbide and Carbons Chemical Company, General Chemical Company, and National Aniline and Chemical Company.

I asked the manager: "How do you do it"?

"Oh, that's easy," he said. "We make only things that we can make at a profit on a small scale and in the apparatus we have.'

"But what happens," I said, "if you get too prosperous and some company starts to make the chemicals you're making"?

"Why, then," he said, "we simply quit making that chemi-

cal and invent something else we can make in our equipment and sell it at a profit."

I turn to another illustration—that of a small company manufacturing a specialty for one of the big mail order houses. It was a gadget the chief engineer had developed on the side. Actually, I don't think more than four men were ever employed in the development of this gadget. Because this company had no research overhead, no permanent organization, and because no special equipment was required to make the gadget, because they had a huge production order, because they sold to only one customer—the mail order house—and because all of these things make a low selling price, they made money. That is, until a competing company by sales manipulation and threat of patent suit persuaded their only customer to desert them, then the company failed.

Then there is a company making automobile accessories. This company in 1934 invented a new arrangement or style of instrument panel for automobiles. Because this design was new, attractive, and had some technical advantages, they sold millions of these panels to two of the larger automobile companies. Being wise in this field, they knew that by 1935 not one of these panels would be sold,—they would be out of style by then,—and the public would want none of them. So, in 1935, they came out with another automobile novelty, not an instrument panel at all. In 1936 they came out with another, and so on to the present. Not everyone clicked but they are still in business in perhaps the most difficult of all fields—that of automobile accessories. They have been successful because they have built their business on a constant research for novel and smart-looking new gadgets. They know perfectly well that what they devise one year will be displaced almost completely the next, because the novelty which made it attractive the first year will "date" it so that the next year it will be out of style—this is the law in a style-

conscious market. Though this illustration has been taken from the field of mechanical goods it is easily possible to find similar illustrations in the field of cosmetics, food preparation, fabrics, clothing, and even in the field of chemicals. The hard-boiled purchasing agent for industrial chemicals, for pharmaceuticals, and alloys would indignantly deny that he is as style conscious as a woman, but anyone accustomed to selling knows that novelty has appeal even in the field of heavy goods and plastics, and that a line leader one year will be largely displaced the next year simply because something has come out, not necessarily any better, but different and novel which has supplanted it. As, for example, molding plastics have largely displaced hard rubber even in fields where hard rubber is more suitable and cheaper.

A friend of mine is associated with a small chemical company doing a specialty business largely in surface active agents. I haven't asked him but I know very well that a large part of the success of his business is associated with the fact that he has been able to come out with something new nearly every year to replace inactive items.

In other words, the small company that is to be successful in research must follow these principles: It must keep equipment and sales cost low and stocks almost zero; it must be prepared to come out every year or more often with something new. This means that its research must be planned well in advance and must concern itself with a field where a steady output of new products is possible. Very frequently this must be a field of its own devising and it must create not only new products but new needs.

More generally, if a small company is to make a success of research, the business of the company must be research. That is to say, research must be such an intimate part of management that as new products develop the company must be flexible enough to change and develop with these new

products. If research is to be undertaken in the small company with any hope of success it cannot be undertaken as a by-product of some special line or as a side issue. It must be the chief business of the company.

## Chapter VII

# THE PLACE OF RESEARCH IN COMPANY ORGANIZATION

In larger companies, especially of the manufacturing type, as contrasted with the investment and service companies, one most satisfactory method of obtaining research is the establishment of a research laboratory as a direct part of the company organization. The first question to be decided is—what part of the organization shall it be?

It is realized that organization is a device for making poor men work fairly well together and that it makes relatively little difference what the organization of a company is if there is proper leadership and proper cooperation. It is astonishing how much the organization of the research activities differs from company to company. One large and very progressive automobile company has no formal research organization at all, but classifies all development activities as design and laboratory, thus splitting research right down the middle. Yet this company has been able to hold its place in the procession of progress, principally because of personal leadership.

Yet there are now some two thousand research laboratories and their accumulated experience has suggested certain principles of organization which it is unsafe to disregard, at least without good reason.

In a previous chapter the distinction of functions of re-

search and other company activities has been pointed out. Experience has shown that it is wise to make the organization follow rather closely the lines indicated by these distinctions of function. In a general way, therefore, research should in most cases be considered as an executive function, because its policies are, in the long run, going to control the future position of the organization.

This conclusion is not universally accepted. In most organizations, the research division is still considered a part of engineering, but the tendency is to promote research to an executive function, as is witnessed by the fact that the most powerful research organizations, such as, General Motors, General Electric, U. S. Steel, etc., have frankly adopted the policy of independent research organization and give to the director of research the title of vice-president. The advantages of having on the Board of Directors and on the Administrative Board of the company one who knows research i.e. the future trends of the business, is so evident that the wonder is that more companies do not adopt this policy.

Once I asked a member of the board of General Motors what the function of C. F. Kettering was on the Board. He said, "A cross between a goad and soothsayer" and that fairly represents, with suitable interpretation, the function of research in the executive part of an organization.

However, research usually begins as a comparatively small group, headed often by a relatively young man. It is not usually considered part of wise policy to give to such a beginner the very important position of vice-president. Indeed, in many cases, the promotion of research to independent status has come as a sort of reward to a relatively old research director as a result of long and meritorious service. The correct spot for research at the very beginning is in the president's office, in a junior position if need be, but directly under the president. But, presidents seldom think of this.

In the beginning a research organization will in most cases start as a department in some existing portion of the organization. If the organization fully realizes that research, if successful, must eventually carry its part of the executive load and if it is realized that research directorship is a training or trial position, it makes comparatively little difference where, in the organization, it starts—that being usually decided by personal reasons.

The actual initiation of research activity in an existing company is usually the result of the vision of one man, who may be a patent attorney, service man, chief engineer, or president. It usually happens, not altogether wisely, that the research, in its foetal stage, develops in the department of that man of vision. This has its difficulties because parents usually demand proprietary rights for their offspring long after the offspring has earned the right to stand alone. But, it also has its advantages, because to a growing but untried research laboratory there is nothing more important than a friend as a sponsor who is high in the confidence of the organization.

It is this that really fixes the time when research becomes a full grown part of the executive organization. When research has, in its own right, earned the confidence of administration, then it is ready to take its part in administration. It must be realized that this is the true function of research, because it is this future position which controls, to a large extent, that most important of all factors of organization, the interrelation of research with other parts of the organization.

The uninitiated look at the lines in an organization from a chart which shows the descent of executive authority and say that that is organization. The wise look at the lines, usually *undrawn* on a chart, which represents the working relations of the various units among themselves

Once in World War I, when the Chemical Warfare Service was first organized, a research man in the laboratory at the American University wanted some information and wanted it quickly. The organization chart said the only way to get it was to talk to the corporal and get permission to ask the sergeant concerning this matter; he would then give permission to speak to the lieutenant in regard to the same, or he would speak to the lieutenant for him. The second lieutenant would then write a letter to the first lieutenant; the first lieutenant to the captain; the captain to the major; major to the colonel; colonel to the general; the general to the chief of the Chemical Warfare Service who would then write to Chief of Ordnance via the Secretary of War; the Chief of Ordnance would issue order to colonel; the colonel to major; major to captain; captain to lieutenant; lieutenant to sergeant: sergeant to corporal, to chief clerk of files, etc., who would give the information requested to the lieutenant who would write it up, sending it via captain, major, colonel, general, secretary of war, general, corporal, major, captain, lieutenant to private, by which time the war would be over.

Or, Private C.W.S. could meet Private Ordnance at the Cosmos Club and ask him.

Of course, few organizations are as bad as this, not even in the army, but it is working liaison between departments which makes an organization function smoothly more than anything which can be shown by heavy lines on an organization chart. Hence it is of very great importance, very early in the organization of a research laboratory, to set up the mechanism of cooperation between departments, while at the same time avoiding overlapping of authority and responsibility which too often have marked relation between research and other parts of the company.

To take these relations up one by one: We begin at the top. As has been said, the ideal arrangement is for the

director of research to report directly to the president. In the event the set up is not made this way, it is important that his immediate chief, say the vice-president in charge of engineering, shall fully understand both the possibilities and limitations of research. This will be the case if the research laboratory is a creation of the vice-president and if the vice-president was responsible for the selection of the director. It will not be the case if someone, say the vice-president in charge of sales, whelped the foundling, which was foisted off onto an unwilling chief engineer. In this case research is "sunk" from the beginning, for it is always possible to kill off research in an organization if one in authority wants to do this, since the results of research seldom show up in less than five years time. This is the most practical reason for placing research under the president in the early critical days of its existence. It should at least be placed under one who fully believes in research and is willing "to go to bat" for it in that critical stage, which comes after three or four years, when the research director must ask for continually increasing appropriations and still has only prospective gains to show to balance the department budget.

The next most important department, whose relationships to research must be defined clearly is engineering, or, as it is often called, development. This is the case even where research is a subdivision of engineering. Perhaps this is the case where it is most important to define the line of demarcation of activities and responsibility and to work out methods of liaison. As has been pointed out, the essential difference between engineering, that is to say product design, and research, is the point of view from which the problem is attacked, rather than the problem which may or not be different.

Suppose I am concerned with the ignition system of an automobile, particularly the storage battery. As an engineer,

or development man, it may have become evident to me that the present storage battery is inefficient. Incidentally, it is. A project, Eng. 211, may be set up to investigate and improve the storage battery for car model 1943. At the same time the research division may have been convinced, also, that the storage battery is not very good and it may, therefore, set up a project, Res. 742 ABC, to investigate and improve storage batteries.

Such duplication of subject matter frequently occurs and has dangers from the point of view of organization. The first thing to do it for the chief engineer, director of research and the two assistants, actually in charge of the work, to get together and talk the project over. Liaison of this sort will make it possible for the two projects to supplement each other, rather than to duplicate each other. In general, the true line of division is, as has been said, a matter of point of view. Properly defined, the engineers project should be, what can be done within the next year to the present type of storage battery to improve performance. As such, the engineers work will include first of all comparison tests of batteries on the market to find the best for the money. He is limited to batteries on the market as, within one year, there is no hope that any radically different battery could be put into production. Second, he may suspect that the battery is electrically overloaded and badly located in the car and that charging voltage and rate should be modified. To prove these things will require tests, which will have to be done on a close time schedule, because of the time factor to be met. He may and probably will soon suspect that the whole battery requires change to be fully satisfactory. For example, he knows just as well as the researcher that the decomposition of battery plates, the need of watering the battery at frequent intervals, suggests the use of some system other than the lead storage cell, but he can do nothing about

that as it would involve a long range series of investigations which go far beyond the requirement of the 1943 model car.. In short, where a complete research department exists, engineers should not undertake projects unless they pertain to a particular well defined product in production, or approved for production, and unless the cost of the project is a proper charge on the development of the product in question. There are doubtful borderline cases, but this rule will work in most cases.

On the other hand, research should undertake on its own responsibility no projects of the engineering type. The words "on its own resposibility" are put in because there are projects properly belonging to engineering, which require special skills or equipment, which engineering does not have. In these cases it has come to be the practice in many companies for engineering or development to ask research to do these. If this is done, it must be done only on permission of the Director of Research and engineering should pay for these not only the cost, but a proper overhead and service charge, exactly as if they were having the work done outside the company. Every attempt to keep down the amount of interdepartmental work should be made, and one of the best ways to do that is to make the charges about that which an outside consulting laboratory would charge for the same service, or a little more. There are cases where research can hire work done by engineers on the same basis, but these are rare, indeed, in the larger laboratories.

So much for the formal matters of organization to be settled between chief engineer and director of research. Of far more importance to the future of the company is a matter of exchange of information.

The researcher, even before he can intelligently formulate his problem on batteries, must have available to him the experience of the engineers actively engaged in the battery

problem. This he can get via the route private, lieutenant, captain, major, captain, lieutenant to private, or he can walk across the hall and get it. Both methods have their advantages and their dangers. The going across the hall method is the only way to transfer those vital half-baked ideas from which progress springs, but it is bad for the record system and it violates an old principle of organization. That is, an organization cannot function if the head of the organization does not know what is going on. Hence it has become recognized as good practice that where actual test data and other formal bits of information are transferred, to do this in writing, copies being sent to the superiors of the men involved. This is a simple matter, but a very important one for reasons of responsibility, patent records and the technical files. But, no system must be allowed to take the place of the personal, cross-the-hall, informal sessions, for it is out of the friendly conflict of point of view that new ideas arise. In most cases this exchange of information of this type is rather one sided, as research is particularly sensitive to need of practical point of view. Often there is rather a tendency of research men not to tell engineers too soon of what they are doing, as such premature disclosure is often thought to lead to dissatisfaction of the engineer with what he has, compared to what he may have in the future and to lead to dangers from the patent viewpoint—or something else.. But, my experience has been that these dangers are rather exaggerated and that depending on the man and his interest, it is often advisable for research to be more frank with engineering associates than is often the case.

The worst thing to do, and this happens much too frequently, is to set up competitive groups in the same organization. Too well do I remember, in the early days of refrigeration, visiting one company where the chief engineer told me with pride that he had separate groups working on

expansion systems, float systems and absorption systems and that he would thus be sure to find out which system was the best. What he was sure to find out was which competitive group had the best salesman at the head.

So exactly wrong is this method that I hesitate to mention it, except that it is still being used in one way or another, and there are still many engineering groups who feel that research is a competitor, rather than an associate. It would be easy to give awful examples here, but the subject is too painful.

We now turn to the relation of research to specification and inspection. This important function of corporate management is often subdivided into two separate divisions, but there is a tendency to group them together as independent departments because of the "umpire" nature of their work. At any event, it is convenient to group them together for the present purposes. Such umpire groups usually have laboratory facilities for chemical analysis, metallographic work and physical tests—in many cases, duplicate facilities are needed by research. Such groups are usually the first to discover defects in the product in a concrete way. For these reasons, the relationship of research to control is often very close. Though the engineer may occasionally use research facilities, it is very common for the control group (to give the group a single name) to use the laboratory facilities of the research laboratory very extensively so that in many cases all of the chemical analyses, are done in the chemical division of the research laboratory. This avoids duplication of facilities. There is a growing tendency in this direction because there is an economic justification for it in the high cost of certain laboratory equipment and the relatively short fraction of time that it is actually in use. Metallographic microscopes, spectroscopes and X-ray apparatus, even in busy laboratories, are used only a few minutes a day, yet

involve investments of many thousands of dollars. Some joint use of equipment is necessary. The question is who shall have charge of that equipment—research or control?

I know no very clear principle to be used to decide in a non-chemical industry whether the entire chemical laboratory should be under control, under research, or whether separate laboratories should be established. In practice, these matters have to be worked out more or less as special cases by the people involved. Basically the matter is economic. In theory, one should be able to justify on a purely actuarial basis whether or not the laboratory will pay better if under research, control, or both, but I am afraid that such an analysis is rarely made. Usually, the matter is decided by personal considerations.

In a laboratory where liaison is well established and where other conditions are right, it makes little difference whether a given laboratory is under research or control, or whether there are two separate laboratories. In practice, conditions are seldom as perfect as this would require. Frequently the analytical laboratories and routine test laboratories of companies, which do not have research control, are rather poor things, understaffed, with poorly paid men, and much over worked. With proper personal approach, the research director will usually find that the much abused chief chemist of the control laboratory is willing and anxious to add to the facilities of the company and will encourage the company to establish a duplicate laboratory free from the responsibility of getting out 100 carbon determinations a day. Such a laboratory is often of great help to him in other ways.

Usually it works out to the best interest to have separate research and routine laboratory if, for no other reason, than to allow proper salary set-up. It is, of course, quite impossible to expect a routine chemist with his stint of "manganeses" and "nickels" to do each day, to double in research. As hard

as it may be to convince the powers that classify employees, there is a difference between chemists and chemists. This difference requires different scales of pay, different employment status, and privileges and different direction for chemists working on research problems and chemists analyzing cement. Research is really a difference in point of view, but that is a profound and important difference which must be paid for and fostered.

In one company, all personnel of the research organization have the prefix "research" added to their company title, so we have research engineers; research chemists. I never inquired whether or not they go so far as to have research janitors, though there might be some advantages even in that.

But, the main point to be made clear, in the relation between control and research, is not the question of who runs what laboratory, but who is responsible for the process and material specifications which are the bible by which control operates. Inspection of purchases for quality of process and also inspection of the finished product to see that it meets standards,—all these depend on a comparison with a set of standard specifications or codes. The question is, who sets up these codes and specifications? The primary responsibility is engineering, but since many engineering departments are really only glorified drafting departments, there is a tendency to shift responsibility for the composition of a given bearing metal, or how to heat treat a given piece of steel, to those who actually work with these things, that is, to the company chemist or metallurgist. As has been said, this dependence of engineering on the laboratory technician is all right if the laboratory man has sufficient imagination to keep ahead of the times; to know what materials are available, or can be created and how heat treatment can be improved. In short, if he, himself, has the research spirit.

There has, therefore, come to be in many large companies, a tendency to require all process and material specifications to have the approval of research, which thus becomes responsible for quality of present production as far as concerns material and process. By this means there has grown up a curious division of responsibility, which can hardly be justified on a purely administration basis, but which seems to work, in some cases. An advantage of making research responsible for the material specifications, by which control operates, which is often cited, is that it keeps the research people in close touch with quality of product and thus gives them the necessary information first hand.

In General Electric and other large companies, this method is not used. Partly because the plants are widely separated and such control of specifications for all the plants is impractical and partly because the system has serious defects. Such divided responsibility is contrary to good organization and it tends both to lower the quality of the design, which cannot thus easily be separated from material specifications. Worse still, this responsibility for material specifications, puts work on research personnel, which is not research at all. The best procedure is to consider the service which research may at times give to engineering in drafting and improving specifications; to be a consulting function of research; to be paid for by engineering, and charged to the job, just as any other consulting work done for engineering is done.

This does not mean that research should not be in touch with process and material specifications. As a matter of liaison, copies of all material specifications should be routed to research. Nor, does it mean that research should not, from time to time, make suggestions to engineers how given specifications could be improved. But, it does mean that the research laboratory should not be considered a glorified combination of analytical chemists, of metallurgists, and other

specialists, whose function it is to control the quality of material and nature of plant operation.

There is but little to be said about the relation of research to production, except the obvious fact that what is invented has to be manufactured. It is the function of engineering to bridge the gap between the idea and the factory, but it is none the less incumbent upon research men to learn in a general way the manufacturing processes. In one company, at least, it is the practice, on hiring men for research, to require them to spend some time in the factory, learning the manufacturing processes employed in that company. Regular first hand contact between factory and research should be provided, because it is just as much a function of research to improve manufacturing processes as to produce a new article. The conflict of responsibility between engineering and research can be taken care of, most easily, by requiring that all suggestions of improvements be made to engineering so that they are carried out through the regular machinery: Research to Engineering to Factory. But, this must not stop the direct contact between factory and research, as far as concerns transfer of information. In short, the suggested flow sheet chart, for ideas, becomes

Research ⇆ Engineering ⇆ Factory
|________ ⇆ ________|

These are matters the smart research director will handle, in spite of organization charts, by building up personal relation with the factory superintendent.

I now pass to relations between research and patents. I am partly repeating in all that has been said before, because the point of view is different. In general, the product of research is things and ways of making things which meet the formal requirements for a good U. S. patent, i.e. they must be novel; they must be useful and they must be such that inventive skill is required.

It may not be that it is advisable to actually take out a patent on the products of research (though it usually is advisable to do so) but it is convenient to evaluate the work of research in terms of patents it obtains or could obtain. This is a hard hearted point of view, which does not appeal to some research executives, but it has its advantages when it comes to evaluation of the result of research effort in concrete dollar and cents terms and, therefore, it is advantageous for the research director to accept this measure of value.

Again in practical terms, this means that the relationship between research and the patent attorney must be very close and rather carefully defined, for the work of the patent department can and has made or broken many research organizations.

In some companies, the patent department is a subsidiary of the research organization. This has its difficulties, because the work of obtaining patents can hardly be separated from the work of defending patents through the courts. This work of prosecution of infringements, protection against suits claiming infringement, arrangement of patent licenses, both as licencee and licensor, are matters which involve the company as a whole and are thus matters of executive policy, rather than research policy. Hence, here it is assumed that the work of preparing patents rests in the general legal department, which is responsible to the president of the company directly.

Now the business of preparing a patent and prosecuting it through the U. S. patent office is a complicated one, involving many steps.

First, someone gets an idea. There is a delightful legal fiction that conception of an idea is a single act to which a date can be put, and *proven*, but in reality, ideas sort of creep up on one. However, if a patent is to be obtained and protected, one must furnish the patent attorney with a writ-

ten record, signed, witnessed by others and explained to others, stating that A on the date B invented C. It takes both humor and experience to appreciate the absurdities and trouble this simple sounding requirement can cause.

Usually there really isn't an inventor. A conception of idea usually requires the copulation of at least two minds. Who in this case is the inventor? Of course, it is possible to file an invention in the names of two or more inventors, but from the legal standpoint, that process has disadvantages which need not be gone into. Since the whole process is unreal, the best one can do is to say that the inventor is the one who said he invented it, i.e. the man who first wrote down on paper and signed his name to what is called a disclosure; but is really a very imperfect description of something which the inventor thinks, in a moment of enthusiasm, may be novel and useful.

Simple as this test is for recognizing an invention, in actual practice, difficulties arise due to one of several reasons. The first is that conception is seldom a complete act. It is, in the case of ideas, a process taking months or years to consummate. What one patents is a process, composition or apparatus described in one fantastic sentence—a patent claim. Now no research man nor anyone else, in his right mind and with proper respect for the English language, ever writes down in his note book exactly that group of words called a patent claim. The birth of that monstrosity is left to the patent attorney, but the claim is the essence of the patent, and the date of conception is the date when the apparatus or process was described as written in the claim. This date will differ with every claim. What date is then to be used?

There seems to be no very clear answer. Curiously the issue has never been clearly defined by the courts, but for commerçial reasons it is wise to date back as far as possible, making sure only that the conception process has no artificial

interruptions but flows on naturally. It is also wise to take the precaution to file as soon as possible, as in a case of interference the party who files first has a very real advantage.

The other difficulty arises from personal considerations. This can be very real indeed. Suppose A first signed his name to a disclosure to an invention which B thought he invented, or suppose A had only the first impulse to mental procreation and that the actual act was done by others. This is often the case. Then jealousies are sure to arise particularly if, as suggested here, rewards are based in part on inventive skill.

Inventions, human nature being as it is, involve delicate matters of human relations. It is the responsibility of the research director to solve these problems or soothe them. The general question of rewards will be taken up later. The best method of solving the question of conception date is keeping proper records and since most technical men are incredibly careless in regard to records, it becomes a joint responsibility of laboratory and patent departments to set a system of record. Such records should be kept in permanent form; not only those ideas which will later be the basis of a proof of conception date, but every activity of everyone in the laboratory which can possibly bear; not only on the conception of ideas, but who was told about it, when and what decisions were made and what actual work was done, so that at some later date, which may be twenty years later, every step in the process of invention can be proven. To see what this means, every research director and vice-president in charge of engineering should take a week off, at company pay, and attend a patent suit where the point in issue is,—which of two or more parties should have title to a given invention. Such suits grow out of interference proceedings in the patent office and sometimes otherwise. It is necessary to go through these things, first hand, to know the impor-

tance of good record keeping in plain dollars and cents.

According to the accepted practice of the courts and patent office, if an inventor is to retain his ownership of an invention, he must be able to prove by written records supported by verbal testimony that (1) *He* was the inventor, which usually means a witnessed statement under his signature that on such a date he invented so and so; (2) that the thing invented at that time is fairly described in the specification of the patent and specifically described in the claim; (3) that he disclosed the invention to others who must testify that he did so, or state on their signature that the herein described invention was disclosed to them at such a date, and (4) that the invention was reduced to practice on such and such a date, which usually means that apparatus was built and operated on such a date and produced the results claimed. The courts have often held that it is insufficient to build an electrical switch, hold it in the hand and snap it (which to any sensible man gives enough information to make sure it will work). It must actually break an electric current, if that is what it is claimed to do. Sometimes it is not necessary to build a machine at all, but in those cases the date of filing the patent is held to be constructive reduction to practice. This is a legal fiction which leads to many injustices. Then it must be shown (5) that due diligence was shown between the times of conception, reduction to practice and filing. Due diligence is defined as continuous activity in the field of the invention between time of conception and filing, exception being made for such delays as are required by lack of facilities and other causes outside of the control of inventor or employer. In practice, therefore, a system of research records should include a completed day to day account of the activities of the inventors and his associates, in connection with the inventions and its variants, including the date of ordering material, date of construction, date of

tests, as well as an account of various variant ideas, foolish or otherwise, which came out of the work. All of this should be signed, or in handwriting and must be dated and supported by the evidence of others.

One of the greatest services a patent department can render research is to set up such a system of records, and to see to it that they are kept. In one very successful company, it is required (1) that every person in research keep a complete personal diary, which is submitted to the patent department every two months, whereupon it is photostated and the photostat dated and witnessed and filed in the company vaults; (2) that every new research project shall be written up in such a way that new ideas, if any, are described, signed copies being sent to the patent department, and (3) that an addition to the diary record, every worker must keep an idea or invention file, or disclosure file, accessible to the patent department and that all material relating to any new idea be filed therein. However, the best procedure of all is to consider that the patent attorney is an *ex officio* member of the research department to be called in for all conferences where new projects are underway, and with free access to all research projects underway.

It is really surprising how many inventions actually originate in the mind of the patent attorney. With his more complete knowledge of the "art," that is to say, the issued patents on a given subject, he is in an advantageous position to know that a given idea is new, which to the engineer or scientist may be so obvious that it is not even worth a patent. So true is this and so difficult and important is the task of actually writing down in legal terminology the exact nature of what is invented that some patent attorneys like to say that the attorney is in all cases the real inventor. I would not go as far as that, but the remark will serve to point to

the very real contribution that a patent attorney can make to research.

This is an inadequate description of the relation of the patent attorney to the research laboratory, but some parts of this subject will be taken up later.

The next department whose relation to research produces interesting problems in organization is sales. The research man is rather apt to treat the sales department as a collection of men high in temperament and low in intelligence. Candor compels the remark that much of the experience of the research director with sales rather tends to strengthen this analysis. Yet, sales of a product is what eventually pays for research, as well as for every other activity of the company. Sales executives are overly fond of pointing this out, —somewhat unfairly. From a point of fact, research support comes from capital rather than income. But, it is undeniable that from sales must come the greater part of those facts which suggest the use of changes in the product, or need of new products.

This is because sales has under its hand the three great sources of dissatisfaction from which industrial progress comes. (1) What is wrong with what is sold as found out by service calls? (2) What improvements the customer would like to have, as discovered by the salesman meeting sales resistance? (3) What effect competitive products are having on sales as is discovered from sales charts?

It is, therefore, a part of wisdom to keep the closest contact between research and sales. One of the many reasons why it is advantageous to keep research separate from engineering and more directly under administration, is to make it possible for the research director to sit in on meetings in a position of equality with the vice-president in charge of sales, so that there will be no artificial barriers between sales

and research. It is often a very useful thing for the research man, who, unfortunately, is not often sales minded, to be educated in the feeling that there is a customer looking right over his shoulder at every move he makes. It might be a good thing if all research men would take a company course in salesmanship.

The practical point is that there must be set up a personal relationship between sales and research executives, so that there flows into research the feeling of what is wrong with the product; what improvements should be made in advance of competition and what improvements must be made to meet competition. This information must flow to research not in the form of written reports, which are too late, but must come to him almost before salesmen themselves are aware of the situation. Like the relation between patents and research, the relation of sales and research must be expressed in personal contacts, rather than in formal lines on an organization chart.

You know research has found success when you see research men becoming welcome visitors to sales conferences; going on golf dates with salesmen, giving occasional pep talks to sales conventions; in short, entering into the informal rather than the formal relationships in which business men get to know and trust one another.

There are admitted dangers in this. Sales is a good friend, but a poor master and the research can often do the service man, or even the salesman, a favor in helping with a tough sale or service situation. It must be clearly understood that research must be paid for work done for sales and service.

Though it has nothing much to do with the issue at hand, I will at this point take the salesman's prerogative and tell a story:

Once I was concerned with the development of a new type of domestic refrigerator—call it the Carnot. This

gadget was operated by heat, somewhat like the Servel Electrolux Refrigerator. In it, as in the Servel, there was ammonia gas under high pressure. This machine included valves and other things that occasionally stuck and also a solid salt, which under some conditions, by a combination of mishaps, could develop sufficiently high pressure to explode the machine. Carnots were more or less experimental, but they had gotten far enough along, so that field tests were desirable. Therefore, several hundred were made and sold, in order to see what troubles would develop, when used in the kitchen of the average home. This was a large company with large business connections. When anything new comes out, news gets around and the executives of big utilities hear of it and often like to try it out in their own home, of course, as a gift. In this case, one of these Carnots was placed in the kitchen of a chief executive of one large southern gas company. Naturally, my company was anxious to make a good impression, as this company, if interested, would be able to sell for us many thousand new Carnots. Thus it was no surprise when I was called in the middle of the night by long distance and told that the Carnot in Mr. Gas Executive's kitchen had blown up and hurt their cook and to get to the airport pronto. A chartered plane was waiting to take me to Georgia. I was only a consultant, but that was an advantage from the company's point of view, as I was theoretically an outside expert called in to view the wreckage (and the cook) and find out what caused the trouble.

Not too early next morning I was calling at Mr. Gas Executive's wonderful southern home on the outskirts of a large southern city. Mr. Gas Executive had gone to work. I had planned it this way, knowing that our real problem was with Mrs. Gas Executive. Mrs. Gas Executive was a gracious lady, not too much disturbed by the fact that her kitchen smelled like a stable (from the ammonia) and looked

like it had been through a flour fight due to the powdered chemical which had blown everywhere. What worried her was Mandy, the colored cook. Mandy was not seriously hurt, but Mrs. Gas Executive was afraid Mandy would be afraid to come back to work. If one has had a real good southern cook, one can imagine what a tragedy that would be. So, we went and called on Mandy. Mandy was enjoying lying on her side on a hospital bed—she couldn't sit up. "Mandy," said I "what happened?" "Well, sah," she said, "I'se a sittin' on my stool in front of that there Carrot machine, peeling, 'taters." (We had already seen the one legged stool of the type often used in southern kitchens. The leg had been cut off sharp by a flying piece of metal). "Then what happened, Mandy?" I asked. "I don' know rightly, sah, but fust thing I know'd de' stool it was knocked right out from under me and as I come down de leg it sure poked me for fair!"

## Chapter VIII

# THE EQUIPMENT OF THE LABORATORY

The previous chapter sets down the general principles governing research laboratories and these principles should be properly understood by all executives before founding a laboratory. Much of the trouble that research laboratories have had has been due to the failure of the management to realize exactly what a research laboratory is and to provide a proper place in the corporate structure for it. These matters of organization can be done best before the laboratory is founded, rather than after. For this reason much attention has been paid to them in previous chapters.

Knowing all these things, the president of the company goes to his Board and obtains permission to found a research laboratory.

The first step in the founding of a laboratory is to hire a director. Sometimes laboratories grow up in companies by a sort of internal concretion, something like a pearl in an oyster. But the chance of finding a good research director inside of a manufacturing organization is something like finding a diamond in Kansas. It has been done, but rarely.

Few companies would build a sales department by letting one of the boys grow up to the job. It can be done, but it is recognized that it is wiser to have the advantage of a new point of view.

So with research. There sometimes is in the organization someone who could be considered as a potential head for research because he is a sort of a "nut" and not of much use where he is. But don't do it. The value of a research man largely rests on the experience he has *not* had in the company.

One is hiring a man, not for what he knows, but for what he can learn, and detailed background in the company and often even in the business is a disadvantage rather than an asset.

This simple fact, that the most important requirement of a research director and of all research men is an ability to look at the company's problems from a *novel* point of view. This has been the reason why so few industrial laboratories have research staffs that are as productive as they should be.

In most industrial laboratories which I have seen, the quality of the men, even the highly paid directors, is sadly below the standards of those few laboratories which which have realized either by sad experience, or instinct, that the true source of ideas is background, and that the more diverse that background the more likelihood of invention.

The wise laboratory will hire its new director from outside, not by promotion. From where? And how to choose him?

There is no school teaching the art of research direction. There should be, but there isn't. At present the usual source of research directors is the faculty of the universities, and research men from other companies. Actually most of the successful research directors were taken directly from the colleges. Coolidge of General Electric; Condon of Westinghouse; Stine, Calcott and Tanburg of duPont are examples. However, it is also true, although it would not be polite to give examples, that the procedure of picking out research

directors from colleges has produced an occasional misfit. It is also true that the successful college professor has by his training and conception of research much to unlearn in industry. Research in the university and in the General Electric Company are two vastly different things; regardless of how much publicity men try to smooth this fact over.

The mortality of college professors in industry would be even higher if they were not allowed to do consulting work and thus adapt themselves to the industrial point of view. If one wants to hire a college professor for research, my suggestion is to try him out first as a consultant, and not judge his ability by what he does or knows, but by how he is able to handle his personal and non-scientific relations to the company.

The research director must be first of all one who understands the processes of invention and who can stimulate and protect them. He need not be an inventor himself, though that is a pretty good test of understanding. Generally speaking, one of the best recommendations a candidate for director has is a list of good patents in his name.

Second, the research director must be a man who knows the people working in the field, from which his personnel must be drawn. Here's where the college professor shines. He has the habit of attending scientific conventions and thus learns to know his peers. Ask on your questionnaire: "On what scientific committees have you served?" Your director is in one sense a member of your personnel department. The success of his laboratory, if a new laboratory, is going to be largely measured by his success in hiring the proper young men; judging, with proper discrimination, the recommendations of colleges and departments from which they are largely drawn.

Third, the director must be able to sell himself to your organization. This job is that of a salesman, or a peddler,

whose stock is ideas. You, the executive, are the customer, as is your chief engineer. Pick, therefore, if possible a man with human contacts, who has hobbies; plays golf; who while in college went into outside activities; who runs the country club and who *has a sense of humor.*

It is hard to elaborate more than this, but all who have hired successful salesmen know these symptoms.

I know that there are many who will be horrified at this and who think of research as being a holy profession, where being an unsocial person is a *sine qua non* of success, but closer experience with the Brahmins of research, Langmuir; Kettering; Steinmetz; Edison; Herty; Midgley, the elder and younger Dow and Baekeland will show that with those with whom they want to work they are charming persons socially.

The research director must sell himself to the president of his company, for without confidence of the organization, no one can win in the difficult, trying and thankless job of building up a research organization.

So far, nothing has been said about scientific ability and practical experience in the field and nothing will be said because the importance of that phase is too much exaggerated.

Coolidge's experience when he came with the General Electric Company to work on tungsten had been with the properties of solutions at high pressures. Condon was known for his work on the theory of spectra. Midgley was a civil engineer. Kettering was working on cash registers. Frolich was a petroleum chemist. Stine was an organic chemist in an agricultural college. Would you hire a civil engineer to work on refrigerants, or an oil chemist to work on rubber?

Hiring a research director, like hiring any executive, takes a divining rod on the side, but short of stealing a proven man from the research laboratory of some non-competitive business,—I can suggest no better formula than this: Line

them all up. Point a gun at them. Shoot them one by one. Then hire the one who said, "Shoot me first," and who just before he died asked, "What was the muzzle velocity of that gun?" You are hiring a man for mental courage; mental aggressiveness and mental curiosity.

Having hired the research director, the next step is to support him by giving him the proper tools with which he can work. Most executives visualize a research laboratory as a building filled with expensive and strange looking equipment—mostly idle. If you ask to see the research laboratory, this is what is shown you. The fact is that more money is wasted on elaborate equipment and fine quarters than can be justified in most companies, except as advertising and publicity. The best work of research that I know of, being done today, is being done in shabby quarters and with borrowed and broken down equipment. Beware of any research director who likes large appropriations for building and equipment.

The strength of a laboratory is to be judged not by the acres of land, or the square feet of working space, but by the number of square inches of frontal lobes owned by the staff of the laboratory. That is, if the frontal lobe is the place in a man's brain where new behavior patterns are established.

The primary job of the director is that of an employment agent. How good he is, is measured by how good are the men he can pick and keep—other measures are subordinate to that.

All large companies have a personnel system whereby they hire stenographers, mechanics, salesmen and other employees. The government with its civil service attempts to apply the same process to its scientific personnel who, in theory, must pass competitive examinations. However, examinations, no matter how severe, fail absolutely to dis-

criminate between research ability and college records. Examinations may and do, in the government, serve to eliminate the hopelessly unfit who, under government rules, can apply. But in actual practice most government jobs of the research type are "fixed" by the chief involved. The usual procedure is for the chief to choose his man by interview, and then write the examination requirements so that only that man can qualify. This is for the higher ranks.

In industrial research, there is no short cut to the choice of personnel short of application, interview and prayer. Some say they can tell research men by the cut of their hair, but I can't. Some other method usually has to be used to judge them, especially for the boys just out of college.

It is quite proper and necessary to choose a research director of the best possible personality. But of the young men and women who must form the body of the research laboratory personality is a weak link on which to depend. Tests of fitness, more objective than the impression made during an interview, should be used.

Some say that there is no way to tell in advance whether a young man just out of college or graduate school will make good in industrial research. This may be true in particular cases but there are symptoms of a good research man which even a too diffident or too confident manner cannot hide.

The first of these is the college record. The best research men are not all A men. That often means single-mindedness of purpose which places all emphasis on the particular subjects studied. The best research men may not even make the best impression on their college instructors. It is extremely important though to get the opinion of the student's instructor (if you know the nature of the instructor). The best man for research is the man with an original point of view which may be shown by the fact that he has dared to deviate from the standard courses and

has taken queer and wonderful electives. One I knew offered, for a degree in chemistry, as minors, mathematics and Sanscrit. It is the student who combines chemistry with botany, radio engineering with thermodynamics who makes good in industrial research. It is the man who changed his school and who changed his thesis subject in the middle, because he disagreed with them, who warrants consideration.

Ask a man about his hobbies and outside interests. A stamp collector I know of is responsible for an important improvement in business machinery. Look for men who have worked or studied summers in related but not identical lines. Don't hire men from your own companies training school. Don't promote from other departments and don't hire your competitors research men on the strength of what they know. You should hire them for what they don't know—and can find out.

Research ability grows by placing men of unusual background and curiosity in a new situation with the incentive and power to act. Research management provides the new situation and the incentive. The laboratory provides the power to act; the training of the men and the novel background. God knows what provides curiosity.

The question of building and equipment is secondary indeed.

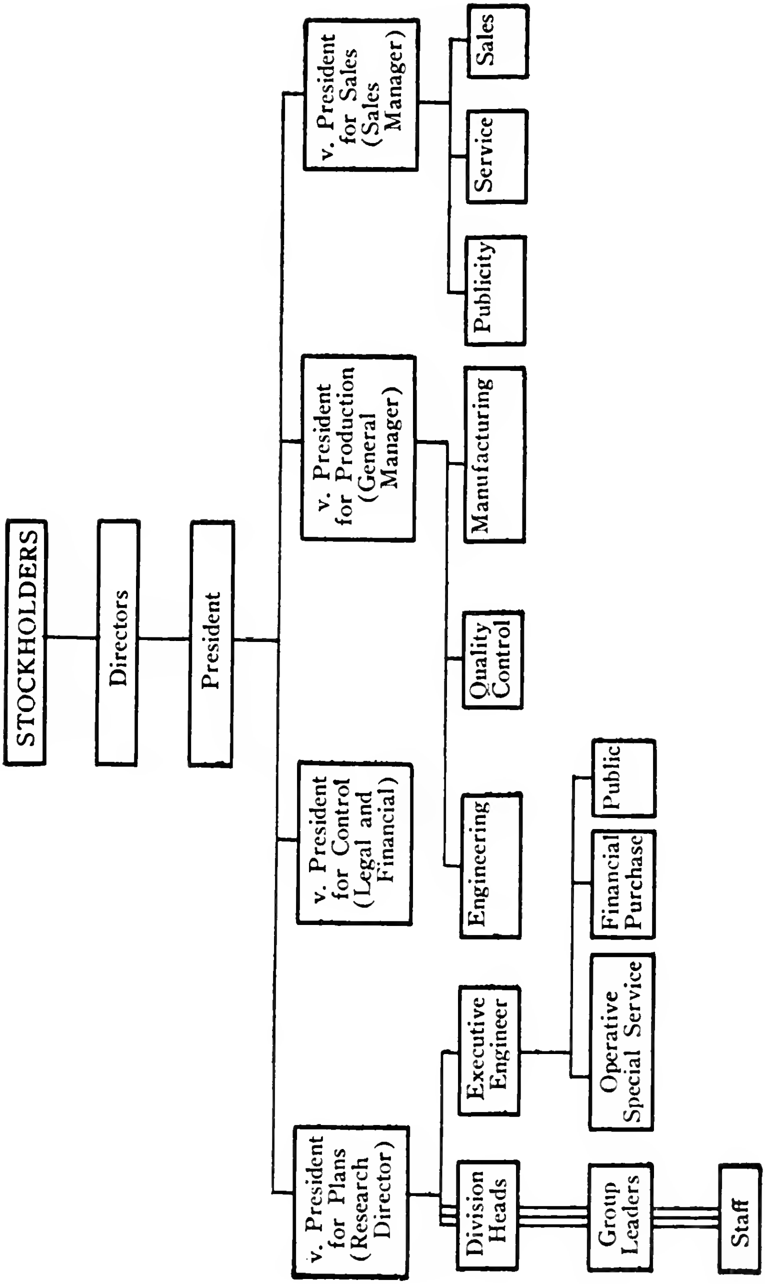
## Chapter IX

# TECHNICAL CONTROL OF THE LABORATORY

The research laboratories which are most successful are, as a rule, organized in patterns which are surprisingly alike. The organization chart of a typical laboratory is shown in Figure 1. This chart also shows the relation of the laboratory to the other divisions of the company. The lines on this chart are lines of authority, not of communication.

Usually, the work of the laboratory is divided into various divisions, each under a division chief. In the best laboratories these division chiefs, meeting together, constitute the director's technical advisor or executive board. The director if he is wise will submit to this executive board all major technical problems which come before him, not to delegate authority but to obtain advice. Under these divisional chiefs come the group leaders, that is, the men responsible for the work on a particular problem. Usually these group leaders, besides being responsible for the management of the problem, are actually part of the working team. Under each of these group leaders and working with them come the necessary aids and assistants who do the greater part of the actual work and often produce the greater part of the ideas.

There are two schools of thought on how to divide, among the divisions, the various problems which come before the


STOCKHOLDERS
Directors
President
v. President for Sales (Sales Manager)
v. President for Production (General Manager)
v. President for Control (Legal and Financial)
v. President for Plans (Research Director)
Sales
Service
Publicity
Manufacturing
Quality Control
Engineering
Executive Engineer
Division Heads
Public
Financial Purchase
Operative Special Service
Group Leaders
Staff

laboratory. In most of the successful laboratories, problems are assigned to the various divisions on the basis of some classification of the problem in respect to subject matter. Thus, in one laboratory, all problems having to do with physics go to a physics division, those having to do with inorganic chemistry go to an inorganic division, and so on to divisions of organic chemistry, electricity, mechanics, etc. Since many of the problems coming before a research laboratory are of the type which do not fall into ready-made classifications, it is often difficult to know in which division to place them. It is the job of the research director to make this decision, basing it on availability of personnel, as well as on the abstract principles of the classification scheme. The great advantage of division of projects by subject matter or project title is that the problems will automatically fall under the complete authority of one man. This man will be the chief of the division to which they are assigned.

In other laboratories, some equally successful, the basic organization is functional rather than departmental. A typical example of such an organization would include a "Division of Fundamental Research" doing the pioneering work on a given problem. Then there would be a "Division of Development" whose function it is to take over the work on a problem where the fundamental group left off, and third a "Department of Operating Research" doing work on the various problems arising from factory operations. Such a scheme has the advantage that it fits in with the physical characteristics of the laboratory in which the groups work. People working on fundamental problems need test tubes and other standard equipment of a scientific laboratory. People working on development problems need test facilities, semi-plant equipment, and the like. People working on plant or operating problems need small or large scale machinery

of the type used in the plant. It is therefore rather natural to divide authority according to tools or facilities.

The difficulty with this system of division is that it is very seldom indeed that a given industrial problem is all fundamental, all developmental, all operational. Neither is it ever quite clear exactly when, in course of development, a problem ceases to become fundamental and become a development problem. This difficulty, however, is not insurmountable. The director can by a fiat put a problem under development if he thinks it is mostly development or if he believes the people working in that division can do the best job with it.

The one thing that must not be done is to divide the authority on a given problem. When it comes to discussion of scientific principles and to the laying out of a scientific program two heads are better than one, but when it comes to managing a research project too many cooks spoil the broth. I have had long experience and I have never seen a single case where a research project was successfully carried on under divided authority. The criterion of successful management is complete unity of authority and responsibility.

The practical difficulty with the scheme of division, according to the type of laboratory function, is that it tends to become divided responsibility. If the fundamental scheme of division is to work, the men working on a given assigned research problem must be able to use the facilities "belonging" to the other departments. The mere fact that "Development" has an Oliver filter press and "Fundamental Research" does not have one is absolutely no reason for dividing the problem of how to make an organic chemical into "fundamental" and "development" simply because the problem requires both a test tube and a filter press.

In most laboratories regardless of the organization scheme

the actual tasks or problems are carried on as what is called "projects." The project is the work unit of the research laboratory. It is some task that is to be done. Usually in order to start any work on a research problem it is necessary to have a "project" set up covering this work. A "project" is a written formal statement of what it is desired to accomplish by the proposed work and how much it is expected to cost in time and money. Usually no work can be done on a project until it is approved by the director and his consulting board and until a definite appropriation has been made to cover its cost. It is then an approved project.

The advantages of the project system are many. First, it gives the director control over the entire operation of the laboratory; second, it gives a basis for record and for accounting of costs and perhaps most important, the project system gives the opportunity for periodic review. Research problems have many lives, and one of the most difficult and necessary activities of research management is to kill them off or graft new glands on them the moment they prove sterile.

All of this division of authority and personnel may seem formal and stereotyped. In certain cases it may become so, but in the wiser laboratories it is usually found advantageous to consider all personnel below division chiefs as a pool from which men can be drawn as needed. In laboratories where this principle is followed the group leader and all men under him are shifted from division to division as the requirements of the research problems at hand suggest and as his talents permit. He is not assigned permanently to any one division. Research is, by nature, fugitive. Problems seldom last more than a year, and even with continuing problems the personnel goes stale, so that it is a sign of wisdom to shift personnel frequently from division to division. It is through this shifting of personnel under a skeleton organization that full ad-

vantage is gained of the different training and temperament of those in the laboratory. When men, under this procedure, cease to have new ideas, give them to production. They don't need too many new ideas there.

To show how this works let us turn once more to the director in his virginal laboratory and outline once more some of the more professional problems that he has to meet. This time, not from the point of view of one who wants to understand generally what research is, or from the point of view of the executive, but from the point of view of the research man himself.

It is not the purpose of this book to describe the various technical procedures used in the solutions of research problems, though there is room for such a book, but there are certain general procedures which influence research management.

As it has been said, inventions arise in all sorts of queer places. In the plant where something has broken down, in a Pullman car coming back from a trouble shooting or service trip, in a conference on something quite different, in bed,—almost anywhere.

To increase the number of new ideas, research personnel must be given much more freedom to come and go, to gossip, smoke and go to conventions, than would be customary in any other part of a closely managed company. They become a privileged or elite corps and this is one of the very best reasons for a separate research organization, since otherwise these "privileges" are apt to cause difficulties.

Suppose somehow, somewhere, Bill Smith gets an idea which he thinks new, useful, and ingenious, of sufficient value to the company so that it should be tested. Usually, the idea will have originated from some half-suggestion made by someone else, but never mind that, Bill thinks it is his and thinks it important, and novel enough to be worked on.

He therefore writes it down in his inventions book, and day-book if he keeps one. He dates this description, illustrates it, if the invention involves apparatus, and signs it. This is the first act in the legal process of invention and constitutes "conception." To prove conception, he has someone witness his signature. But that is not enough, he must disclose his invention to someone sufficiently trained to be able to understand it and not a party to the invention. Bill therefore goes to Bob and says, "Bob, read this and if you understand, sign my book, saying on such and such a date, I read pages P to Q of Bill's notebook and understand the invention therein described."

That meets the legal requirements, but writing something in Bill's notebook and describing it to Bob, won't get Bill any further. It will just be words in his book unless Bill does something to interest the laboratory authorities in the idea. He therefore writes a memorandum (we are assuming that this is an ideal laboratory). Bill sends his memorandum in duplicate to his group leader if Bill's idea came from what he was doing, but not necessarily if the idea is extracurricula. If the memo looks like an invention which might be patented, Bill's chief will read and witness it, send the signed original to the patent division (the first thing before he misplaces or loses it). He will then call Bill into conference, bringing in any one else from director to office boy who may contribute, —no holds barred.

Usually the "idea" does not survive this informal conference. But, there may arise, out of the process of killing it off, another idea which seems within the range of possibility. If the conference seems like one from which an invention might arise a representative of the patent group should sit in, but in any event the conclusions of the conference, the persons present and the subjects discussed should be sent in memo form to the director, patent attorney and others concerned.

Sometimes, a more permanent New Devices Committee is set up to handle such matters. It is a great advantage to have such a committee as it gives a means for handling, in a uniform manner, extra curricula ideas arising outside a man's regular duties, as well as ideas arising within the company outside the research laboratory. It also allows a discussion of new ideas from a broader point of view than that of the group leader and departmental chief. It serves to unify the activities of the various divisions but it must be thought of as supplementing and not supplanting the informal conference of Bill and his chief. Action arises not from groups but from heads.

Out of this conference may arise the suggestion, concrete enough, to form the basis of a definite project but usually what arises is questions. Nevertheless, this conference must be written up for it forms an essential step in the important process of "due diligence," the lack of which may deprive the company of the fruits of Bill's conception.

The next step is the crucial one. The answering of the questions takes time and time costs money. Until the questions are answered, no one knows, except in the most general way, whether the idea is any good.

At this stage, the idea is submitted to the director. He will or should turn it down because a project is a unit of expense and must be justified as any expense item should be justified by an estimate of time, cost, probability of success, and probable income; but these estimates themselves can't be obtained without expenditure of time or money.

There are three ways of handling this procedure. The first and most common is "bootleg research," that is to say time and or laboratory supplies are expended by Bill (with the knowledge of his group leader) and this time and supplies are charged on whatever going project can stand this extra expense.

This process is frowned on by all laboratory executives.

Nevertheless, it is necessary unless some better means of handling this needed preliminary experiment and library search is found.

The second method, or "slush fund" method, is to allot to each division a certain fractional appropriation or overhead as a "slush fund" which the division head can allot as he sees fit without need of specific authority or formal review.

In practice, this is even worse than the first method because expenditures which do not need authorization review become the happy hunting ground of the players of hunches, at least, those players of hunches who "get to" the division chief in his moments of weakness.

The third method is the method of the Bookmaker. In this method a small sum is set aside, say 5% of the laboratory income to finance preliminary time and equipment spent in investigation of possible formal projects. Usually this fund is given over to the New Projects Committee or its equivalent. To obtain use of this fund, individuals or project directors apply for grants, usually not more than $500, which serve to carry the expense of search, mathematical investigation or very limited experimental work. The purpose of this grant is to investigate, in a preliminary way, the facts needed to set up an intelligently supported project. "Slush" grants are usually made on faith but never without discussion and definition of exactly what is to be done.

The advantage of this procedure which seems formal, is that even in the earliest stage, where it is most needed, the worker gets benefits of advice from people of a wide company point of view, and adequate well supported records are kept.

Because such funds are limited and because the new projects committee has to justify itself later backing these horses by a reasonable percentage of winners, this system allows

individual initiative in originating projects while at the same time, allows review and consideration by others than Bill and his immediate superiors before the laboratory funds are expended.

Let us suppose Bill has obtained (or taken) time and permission for his period of preliminary labor. This preliminary work will usually include a literature search and probably some very preliminary experiments. It will be done under a strict allotment of time and money. Penuriousness, at this stage, is a virtue as it is the best known incentive to carefully planned work.

The next step after proper records have been made and witnessed (I shall repeat this phase *ad nauseam* in what follows) is for Bill, having obtained permission, to undertake a literature search. Laboratory work is expensive not only in direct cost but also in time. It is usually advisable not to repeat work done elsewhere and published unless there are very special reasons for so doing. The library is thus a primary research tool, and the size and completeness of the library is a much better test of the quality of a research laboratory than miles of laboratory tables and great displays of test equipment. Investment in the library is one of the important elements of expense in the equipment of a modern research laboratory. There is another reason for preferring to look up something in the literature rather than doing it in your own laboratory. The sad fact is that the work in industrial research laboratories is seldom as well done as that which is published by universities and scientific organizations. There are, of course, brilliant exceptions. Some of the best work of this kind, that I have ever seen, came from the Bell Laboratories.

The library search, with which all should be familiar from their college days, should include not only the scientific literature but also the patent literature. It is this search which

forms the basis for the formulation of what is patentable in the new idea. All such searches should be carefully recorded and a copy, even if nothing is found, sent to the patent section, provided the subject matter of the search has at any time been thought possibly patentable. If Bill doesn't know how to make such patent searches, he will ask the patent department to show him.

With the search completed, it is usually possible to do some design or process calculations. These will often show that the idea won't work for reasons of cost, size, lack of efficiency and other "numerical" reasons. These preliminary design sketches and calculations should be again carefully saved and sent to the patent attorney. They form the real date of conception of the apparatus as actually claimed, since the original idea is often too broad to form the basis of the claims as allowed. The claims, in the last analysis, form the patent.

At this stage, if not before, Bill or his chief will be in a position to write out a formal proposal for funds and time to continue work on his idea. This is the request for a project. This request will include a careful analysis of the project from as many points of view, and as factually as possible. It should include the amount of money, the time, and the number of men needed. Bill may not have a basis for all this but his section chief and superiors will. It will also include an outline of just what the project aims to accomplish, and what steps are to be taken. It will show the relation of the project to other projects, and define the boundaries between it and other projects. It will include an estimate of the value to the company of the gadget or process in dollars and cents (if it works) and what will be found (of value) if it doesn't work. This estimate can be in terms of total business profits if the company evaluates projects in that way, but in the system advocated here, should include

the selling price of the idea if patentable; that is to say, its capital value. The relation of capital value to profits, gross sales, depreciation, probability of success, required capital investment and amortization rate, etc. is usually a thing Bill won't know how to calculate but the accounting or business departments will know how and it is Bill's responsibility to get these figures from them.

Various formulas have been proposed to help evaluate research projects but that differs from business to business and so will not be included here.

Having written up his project, Bill will send it via his superior to the director. If the request is well done and if the section chief has prepared the way properly, approval is or should be almost automatic except in certain cases.

The cases where approval is not automatic are the very new projects involving large matters of company policy, or large expenditures of funds. These will be discussed by the director with his executive board and with other divisions of the company if need be. It is a matter of courtesy and good judgment to invite Bill and his superior to be present when the decision is being made.

The project is now approved, usually for less money than is requested, not only because laboratory directors are, by nature, penurious but because a good project in a new field will project the work far enough so that the director can estimate the total probable cost. It is usually possible to break down the work as outlined into a series of steps, and it is wise, in the early stages, to appropriate step wise, for reasons of control and economy.

Thus, Bill will get only a tenth of what he asked for, but that is enough to start to work. At which point, Bill will swear off for the day and go out and celebrate.

In some laboratories, the section chief is responsible for keeping within the allotment of time and money. More usu-

ally it pays to have, under the director, a business manager who actually handles all financial details involved in purchasing and time keeping, thus freeing the department heads from any responsibility save towards the progress of the work. The job of business manager sounds like a job for a stenographer or a junior clerk but it can be far more than that as the example of Hawkins at the General Electric Laboratories proves.

The actual mechanics of financial and time control over rapidly shifting projects of a research laboratory involves judgment and the ability to get along with temperamental and very insistent personnel which mark financial management in its very essence. More of this will be worked out in the next chapter.

Bill having concluded his celebration now begins the actual laboratory work. This work is usually of two types, often done simultaneously. The first is the building of a working model, if the invention is a device, or carrying out the process on a laboratory scale if the invention is a process.

This step, often done crudely and with makeshift apparatus constitutes reduction to practice and must be observed by others than the inventor and must be carefully recorded and the record sent to the patent department. In one laboratory it used to be the practice for the patent attorney in charge of the case to witness such crucial experiments.

Behold the invention works, but is not feasible! This is the usual result.

Then begins the real work. The experiment is repeated, this time under test conditions. Thermocouples, gages, and other measuring gadgets cluster all over the device. People takes its temperature, pulse and even more intimate characteristics. This is the stage where one gets test data on which design must be based.

In the meantime, further work is going on in the labora-

tory on the elements of the design including investigations on the properties of materials to use, on the conditions under which it should operate and be manufactured. Variants should be tried and tested. This is planned research and there is very little chance to it. It is also low grade research for most of the laboratory work, at this stage, consists in using well known methods to determine properties of materials, working methods of analysis or test, and making well known tests. Little of it requires any higher grade of intellect or breadth of experience than that of the average graduate who has done well in a university laboratory. Hence, it is usually done by such persons under the observation of those who might see any surprising or unexpected result.

This supervision must never be neglected, for invention has a habit of budding or producing sports as the biologists call it. Laboratory investigation, undertaken for one purpose, often suggests ideas quite different, ideas which may be overlooked by those without a broad point of view. Hence, it is unsafe to leave anything being done in the laboratory without a supervisor present. Incidentally, this helps when it comes to records.

Of course, there are inventions which do not follow the type we have been describing. Thomas Edison had been quite unfairly considered the inventor of the "try-every-bottle-on-the-shelf method." This method had to be used occasionally but even a little planning of the proper order in which to take the bottles down will save time and money. A laboratory can be largely judged by the planning of its research. In these days discoveries are practically never the result of accident.

There is a third kind of research which seems to many a sort of holy rite to be spoken of with bated breath. This is PURE research.

The truth is that there is seldom justification for an *indus-*

*trial* laboratory to work on problems for which an immediate commercial need is not seen. To work on soap films just for the fun of it or to increase the sum of human knowledge is not good business or good sense except under certain conditions.

To justify pure research requires the same reasoning that is required to justify the most impure research. The difference is simply in the factor of time. Langmuir played with poorly evacuated bulbs because he or Whitney (in this case both) knew that new methods and new ideas were needed if lamps were to be improved. They didn't know what these ideas were. Otherwise, they wouldn't have had to do this. They worked in the general field with the exact spirit with which Edison took down every bottle on the shelf, so that something would have the chance to turn up.

Pure research can thus be poor research, in the sense that it is born of desperation after everything intelligent has been tried. It may be poor research done by a *very good* man. Only when clear cut, curious, practically-minded men like Langmuir do it, can it succeed. Soap films are worked on at the General Electric Company because new theories and new methods of attack on all problems involving surfaces are needed. These methods and theories can come out only by the accident of an aberrant experiment in the hands of a brilliant man.

Pure research is justifiable in the industrial laboratory principally to open new methods of handling problems seen only in general terms. Its purpose is not to find new data or new processes but to find new techniques. Theory is a technique.

So much for the problems of the laboratory. How the research worker is to handle situations like "Mandy" I know not how to formulate.

## The Financial Control of Research

Some books on research would indicate that research does not cost anything. The fact is that measured in terms of the usual business standards research is an expensive luxury. You can pour in a quarter of a million dollars and have the research director come smiling before the board of directors and say, "Well, gentlemen, that proved to be the wrong hunch."

Because of the speculative nature of the investment in research there is a tendency for boards of directors to pour in money when business is good, and to cut it off entirely when business is bad. Laboratory directors remember the dictum that came from the higher ups in the dark days of 1931—"Cut out work on all projects not producing profit."

The only cure for this is a proper financial policy for the laboratory. Here is where a good business manager can be worth his weight in gold and more.

Basically the difficulty in financing laboratories comes to a matter of bookkeeping. The actual expense of the laboratory (seldom over one percent of the gross for a large corporation) is never enough to worry the directors provided they have a tangible method of making a budget. The trouble is that if the bookkeeping is set up so that all the items on the budget are on the red side (as is apt to be the case in the standard bookkeeping systems) there is no way of exercising proper financial control.

Most laboratories have three courses of income. The first of these is charges for work done for other divisions of the company, and it is the essence of good bookkeeping that these charges be made a real charge on the divisions for whom the work is done. The next source of income is outside royalties. Some companies refuse to license competitors,

but this is usually very short sighted. One of the reasons is illustrated in Kettering's famous remark "The best way that I know of to get General Motors to use one of our inventions is to license it to a competitor."

Indeed, a very sure test of good company policy toward the research laboratory is that it understands that the research laboratory is a selling organization—its goods being patents. Once that realization has been reached, a large part of the problem of financial support of the laboratory is solved.

In some laboratories this policy of licensing patents goes to the extent of actually charging their own manufacturing divisions royalties for the use of inventions made by their research laboratory. This policy has been recommended by Swann. I have had no actual experience with how it works in practice, but some equivalent must be had if a factual method of budgeting is used. The theoretical difficulty with it is that what is produced by research is wealth, not income. It would seem to me, therefore, that a proper research budget should include not only income items, but capital items and that a procedure should be set up to evaluate the patents owned and issued to the company. This value should be the dollars and cents value that the company would be willing to sell the inventions for outright. Of course, it is difficult to determine this selling price, but not impossible, for companies do sell and buy inventions. The procedure I should suggest would be for research to set up an asking price and that divisions using the invention approve this price before using the invention, paying not as a royalty, but as they would finance purchases of new building and equipment.

This is somewhat more complicated than putting research on an annual basis, but it has advantages from a tax standpoint, from the point of view of stability of income and most of all, because such a procedure keeps clearly in the minds

of the directors and of the investors and of banks, the real position of research in industry.

One sometimes has to borrow to cover plant expansions made necessary by new products. A proper and standard procedure of accounting, showing the value of inventions as security for capital is badly needed in these days, where security for investments can hardly be based solely on buildings, land, and cash position.

One of the biggest things a research director and his financial associates can do for a company is to set up such a system.

This is on the black side of the laboratory budget. The red side also has its problems, because of the fact that almost all expenditures are special and it is seldom easy to set up and budget laboratory expenses. Much more leeway must be given than in manufacturing departments. Who knows but that on January 5th someone may get an idea which will require unexpected expenses of $50,000.00 immediately. Nevertheless, one can't set up budgets which will allow research to draw company funds as needed.

It is a part of good business and an essential part of the buildup of confidence that management has in a research director, to keep expenses as low as possible. This can be done provided a close system of accounting is kept and provided the expenses of individual projects be kept well below the approved figure. It is on the group leaders that this responsibility really rests. One of the essential points of good financial management is that the project chief be made responsible not only for the work, but for keeping the work within the allotment, and that he be given credit for saving made by shrewd management. Many projects can be carried out at a fraction of estimated cost.

The major difficulty is overhead. Many companies impose, by their accounting systems, a uniform overhead for

all departments. At first sight this is an advantage to research, whose actual non-distributed costs are usually high, but it is better in the long run to insist on keeping separate overhead systems and in keeping it down to the absolute minimum. The next and most important duty of finance and the real reason for this chapter is the question of promotion, salary and special awards. In the long run a satisfactory program of handling personnel problems will make or break a research laboratory.

The first thing to see to is that the salary scale of the laboratory is not restricted by the general salary policies of the company. A chemist doing research is quite different than a chemist in the routine laboratory and it must be the policy of the company to allow pay scales based on the circumstances of the case, rather than in accord with so-called company general classifications. The easiest way to do this, as has been said, is to classify all research men, as research chemist, etc. But, it is promotions and special awards which wreck laboratories. A man who has made a valuable contribution to the wealth of the company, often in value many times the whole lifetime salary of the man, should be rewarded right then proportionately to the value of the invention, as soon as the value can be determined.

The usual system for doing this, payment of cash or company stock for every invention made, is hopelessly inadequate and unfair. Inventions differ in value. It is unfair to reward the nominal inventor disproportionately to the work of his associates. To do so puts too much emphasis on the first phase of invention and tends strongly to prevent team work, without which a laboratory cannot long prosper.

The best procedure of which I know is akin to the evaluation of patents, previously mentioned. This involves paying to the laboratory, for distribution among the personnel, a lump sum as a bonus in proportion to the wealth created.

This is distributed so that all in the laboratory, even secretaries and janitors profit—those most responsible profiting proportionately. One procedure is to give to the group involved in the actual work one-third, one-third to the laboratory and one-third to administration to be distributed as it sees fit. Whether the actual payments are made in a lump sum or distributed over years is a detail.

All this is in addition to normal salary raises, promotions and general company bonuses This procedure is more or less ideal, but based on the realities of the situation. Inventive work should be properly rewarded in a way proportionate to what one would have to pay for the invention if bought from outside companies.

The advantage of this is that it keeps before the laboratory personnel the incentive to invention, the danger being that the money so created by the laboratory be not fairly distributed. But this danger is present in every method of award, and there is no formula known to me by which one can tell just how, in a given case, to distribute salary or bonus.

There is however the good old fashioned principles of award in proportion to merit. Distribute the bonus so that no one is omitted—even if he is not actually involved in the invention—that may not be his fault—it usually isn't. Distribute the major portion to those most actively contributing to the invention made and that may not include him whose name is on the patent. Pay due regard to the fact that departments of service, the analysts and test men are to be rewarded. They too contributed. But never forget that those few who really contributed the ideas, the force and the skill back of the successful product have earned a special proportion of the award.

It is the job of director, financial man and section chief to work this out in real fairness, so that the laboratory will

work as a team yet have the incentive to brilliant personal effort. If your director cannot do this fairly, fire him quickly before he breaks the spirit of the laboratory.

Some company executives object to any special awards on the ground that a company hires a man to invent and provides for company ownership of projected inventions in the contract, and that that should close the responsibility of the company to the inventor.

Legally, it is not quite certain that this is so, as there are many cases where inventions made by company employees, even working under a contract to assign all patents to the company, are held to be the employee's property. The general principle is that a contract to perform certain services must specify the services, and must include payments in accord to the value of the services. An inventor hired to invent improvements for a given refrigerator, must assign on agreement title to those improvements he makes on a given refrigerator. If the company makes electric stoves and if the inventor hired to make improvements on refrigerators invents an electric stove, the stove invention seems to belong to the inventor, in spite of the fact that the assignment contract may have included a list of all interests of the company, electric stoves included. It is not the contract form which really governs, it is whether or not the invention came as a consequence of what the inventor was hired to work on and actually did work on, in which case in reality he was not the inventor, or whether it came outside the course of his actual responsibility.

This is not quite certain, because of conflicting decisions, but it is a safe law, as well as good personnel management to recognize the proprietary interests of the inventor in his brain child.

This is aside from the question of actual royalty income from inventions. There is a growing tendency, and it is a

wise one, to realize that where an invention is licensed outside the company, that the inventor should share directly in the royalty income. This is because, in this case, the company is in effect the agent of the inventor.

All of these procedures, although more or less formal, and subject to special exceptions, are preferable to the sort of personal informal promotional system which causes so much jealousy and real injustice in many laboratories.. Promotion is, indeed, a poor way to reward an inventor. Promotion in the laboratory should be based on the ability of the person to perform the function of his new job, not on what he has done.

Perhaps General Electric was the first to realize this and, although handicapped at first by the lack of an intelligent system of rewards, they have given to certain men huge salaries without at the same time promoting them. That is to say, without requiring their time on the executive activities that normal promotion requires. Such men may often remain as group leaders (in the classification of this book), yet they may be paid much more than most department heads.

But, this queer procedure is only a way of doing something which should be done by a proper system of special awards.

## The Research Laboratory and the Public

Let me tell you a story. When I was a young man I made my first trip East. In fact, I stopped off to see Dr. Langmuir, whom I had met, and was courteously invited to lunch at the Mohawk Club. At an adjacent table was Dr. Whitney who was entertaining two gentlemen, whose names I never did get. Dr. Whitney, with that characteristic consideration for which he is known, got up, came to our table

and invited me and, of course, Dr. Langmuir, to join him. I had met him only that morning. The two guests of Dr. Whitney were, evidently, executives of the company. They were engaged in an animated, though only half serious, discussion on the value of research. I thought that Dr. Whitney was on the defensive and according to my inexperienced ears he was getting the worst of it. "But, you dón't count," said Dr. Whitney (of course, after thirty years I don't quote exactly) "the value of the business that the Tungsten lamp has brought us, or Coolidge's X-ray!" "Business?" snorted the older of the two men, "why we've sunk thousands into Coolidge's toy and I doubt if we will ever get a penny out of it. And, as to the lamp, that has involved huge plant expansions which have, as yet, to pay a profit."

The men laughed. They were all good friends. I felt that I ought to say something, so speaking rather diffidently, a habit that I have since well outgrown, I said, "But, haven't you forgotten the advertising value of the laboratory?"

They all looked at me. Then, the older man put his hand on my shoulder and said, "Boy, you've got something there. Do you know how much we value Steinmetz? One million dollars." Then turning to Langmuir, "I'd hate to say in front of this young man how much we value him, but this is for advertising alone. Advertising alone!"

In fact, a research laboratory properly promoted is worth a huge fortune to the company. Steinmetz, Edison (whose name was appropriated rather than bought), Langmuir, Coolidge, Whitney, The House of Magic, have become the symbols of scientific and technical leadership and superiority of quality (not always properly deserved). So that to the man on the street and more important, to the college professors who teach the men who will soon buy their products, the mark "G.E." has become a sign of quality.

Other companies have followed in the trail, but still there

is much more than can be done to capitalize on the publicity value of the research laboratory and on the reputations of the men who direct it.

In this same connection is the question of the publication of scientific results. General Electric and Bell Laboratories, besides contributing many scientific articles to standard journals, publish first class technical journals of their own and encourage attendance of company personnel at scientific meetings; both as listeners and to give scientific papers. Much is often made of the fact that companies publish scientific papers as a means of encouraging the publication of results by competitive laboratories and that the company sends its personnel, at company expense, to such and such a scientific meeting to listen to the papers and thus learn what is going on. The real reason is to have their research and technical heads constantly in front of that part of the purchasing public whose opinion counts most in deciding whether a General Electric or a Westinghouse motor will be bought, namely the technical and scientific personnel of the big buyers.

In principle, to an industrial organization, a scientific meeting is no different than a grocers convention. It is a show to impress the customer.

But, there are other ways of impressing the customer. One of these is through sales. The story of Mandy illustrates one aspect. The fact that a huge company sent a "famous" (?) scientist down to investigate an accident and, incidentally, got Mandy back to work, turned an accident into a source of good will.

And, although General Electric hasn't, as far as I know, used Dr. Langmuir in this way, they have used others nearly as well known. The use of real or imaginary research laboratories to back up merchandise, either in case of failure, or as an aid to direct sales in very special cases, has an im-

portance to company prestige and to the self-confidence of the salesman that cannot be exaggerated.

Particularly is this the case where the goods are built to order competitively, or where untried features are involved. The Surface Combusion Company, for example, whose business is largely in heat treating furnaces involving new and often unproven processes, finds it almost necessary to send the scientific personnel along with the salesman, because such new untried apparatus can be sold primarily on the confidence built up in the customer by the special technical knowledge of the process shown by the inventor.

As far as selling patents is concerned, such special sales procedure is an absolute necessity. But, the Research Laboratory should be paid for this service.

We finally come to the relation of the research laboratory to an important part of the public, the competitor. "There is no asset to a company quite as valuable as a good competitor and every means should be taken to foster and encourage him." I don't know who was responsible for the mock serious sentence just quoted, but this sentence should be on the desk of every company executive, for taken rightly, it represents a most important point in the philosophy of business.

Reverting to the point of the first chapter, patents and researches are a device for the destruction of a competitor. Sometimes, as has been shown, they actually work that way, but only when the enemy is unprepared. An illustration from modern war will illustrate. Given air control, the airplane can destroy enemy air bases and even weak enemy air forces, almost at will. It is, therefore, wise to use it thus, as has been brilliantly shown by the Germans in the Aegean, Norway, Poland, France, Belgium and Greece. But, once the enemy can match you, the picture changes. One can do great military damage with planes. Still if the enemy can

use planes at an equal force, he can do damage to you equal to what you can do to him and there is no profit in that. Hence the result of an equality is that it tends to decrease the use of planes on both sides and that holds until one side sees that the damage it can do is very definitely greater than the damage it will suffer.

Research laboratories play about the same role in business as planes do in war. If you can clearly put your competitor out of business, because of a new product, you do so. But, if not, or if you fear the competitor might have an ace up his sleeve, you treat the enemy as a gentleman and sportsman and don't try to press home, too far, any advantage you may have.

Abandoning the military analogy, it pays to take this attitude. Patent suits and infringement procedures are expensive and unsatisfactory. In the end the customer will want and should have the good features of your competitor's product, as well as yours.

Hence, there is an increasing tendency, without which the automobile business could hardly live, to trade patent licenses and even trade secrets, at least between large companies.

Legally, this is a very delicate subject. Patent pools, which are the natural loci for patent trades, have "a very bad smell in the nose" of the modern court, it being held that they are measures for restraint of trade. So what? The social aspect of it, it seems to me, is that they are the best known means of assuring that the customer gets, whenever he spends his money, the best product that can be obtained. Of course, some little fellows may be driven out, which is bad for them, but on the whole, good for the customer, for as a general rule the little fellow is weak on quality.

It's lots of fun to write this paragraph, because there are so many "socially" minded people who take the other posi-

tion. But, right now the net result of the prosecution of trade associations, patent holding companies, is that the question of cross licensing has to be handled with care. Unless, indeed, the simple thing is done and patents are individually licensed on a royalty basis. It is, of course, silly for General Motors to pay Chrysler one hundred thousand dollars a year for a license and for Chrysler to pay one hundred thousand dollars per year to General Motors for licenses, but it is legal and it has the advantage of giving a means of valuing patents.

As far as keeping secrets from your competitor, I remember remarks made to me early in the first World War. "The purpose of military secrets (and the same is true of trade secrets) is to keep the enemy in the dark about how little you know." And, an experience: I asked the executive of a large company to give me his formula for a certain composition he was making. This man said, "I'll give it to you as I must, as you are working for the Government, but I will have to have permission of the Board of Directors, as that is a rule of the company. It will be much simpler for you and save me a lot of trouble, if you will go to So-and-So, a competitor of mine, and ask him what our secret formula is. He'll tell you offhand."

I can close this book in no better way than to tell a fable.

Once upon a time there was a nation of woodchoppers and they lived in a forest where there were many trees, strange to say. They were very happy because although they had a King by the name of Ki, they had enough wood to chop for their own use and enough wood to give Ki all he wanted (which was a lot).

Now Ki had three sons and their names were Ar, which is short for Army, and Ind (short for Industry) and Re (short for research). The people, liked Ar and Ind,—and Re was so small he didn't much count.

Then one day there came to the edge of the forest a great dragon and he did lots of damage, swinging his tail, for he was so big and powerful, every time he swung his tail many trees fell.

So, the King Ki sent out his son Ar to destroy the big bad dragon. But, when Ar got near the dragon he found he could not do very much because his sword was too short, but he did his best and got in one or two good digs. Now, this made the dragon angry, so it thrashed its tail some more and turned on Ar and drove him back further and further into the woods and everywhere the dragon pursued him more and more trees fell, for the dragon had a very big tail.

Now, when Ki saw this, he called to his son Ind and he said to Ind, "Behold, how it fares with your brother. Go you into the forest; burn them into charcoal, dig up some ore and make a bigger sword."

Ind was big and powerful, but he was kind of dumb, so he said, "Behold, I know not what kind of a sword to build. Send me my brother Re to help me." And, Ki said, "No, no. Not that. For your brother Re is small and unpopular and controls no votes. I will create for you a committee with lots of big names."

So he did so and Com told Ind how to make a sword which when swung was so big that it would surely kill the dragon and incidentally cut down many more trees.

And, Ind made the sword and Ar took it and with it attacked the dragon with lusty blows. I'm not sure whether he killed the dragon or not, but they fought all over; anyhow between the swinging of the dragon's tail and the swinging of Ar's good sword, almost all of the trees in the forest were killed.

But, it made little difference, for the real Re, to whom nobody paid much attention, had followed the fighting and

everywhere a tree had fallen he had planted a seed. So, when the fight was over, years later, there were as many trees in the forest as before, only they were better and bigger and bore much golden fruit.

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